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CIVIL ENGINEERING.

The Construction, Traction, Retardation, Safety, and Police of Railway Trains. By W. Bridges Adams.

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Wagons and carriages are kept short on railways in order that they may roll round curves. Viewing a train of wagons from a bridge, every wagon will be seen to oscillate from side to side on the rails, following the course of curves and irregularities. Every wagon is drawn by a loose coupling chain, some eighteen inches in length, and the oscillation is so violent that, though goods and coals suffer it, and suffer from it, it would be unendurable by passengers, so the carriages of passenger trains are close coupled together to keep them steady. But in this process the wheels, as constructed, are debarred from following their own courses and are compelled to slide. The result is a great wear of flanges and of axle-brasses, and a large consumption of coke. Were a train of goods wagons as close coupled as the passenger trains it would be simply impossible to move them. The first thing an engine-driver does, when about to start a goods train, is to back the whole of the wagons one on to another, and then start them one at a time in succession, by snatch after snatch at each chain, which is therefore required to be of enormous strength, in proportion to the resistance of the vehicle to traction by reason of bad structure.

The necessity for close coupling the passenger trains, arises solely from want of the efficient structure to induce free movement to make

each carriage tractable instead of resisting—docile and not wilful. The first thing is to attain great length to prevent pitching movement, just as is the case with vessels on water. But with long vehicles, radial movement of the axles must be attained, as described in a former paper. Radial movement may be obtained by radial axle-boxes to the end wheels very effectually with one pair or two pairs of central wheels to serve as fulcra on the rails, or with frames of iron fixed to the axle-boxes and guided by the traction rod and buffer. The traction rod in such cases will serve in the same mode as a carriage pole on the highway. And swivelling buffer springs will, by coupling chains attached to the iron frames, keep the wheels in the right position while backing the train on curves. Or, if two pairs of wheels, coupled together at each end of the frame, be arranged with a quadrant or a pivot over one axle and a radial curved bar over the other axle of each pair, they will be self-guided on the rails, and a carriage thirty feet long may thus roll round a curve of fifty feet radius, and the steadier will it run, and the less will be the likelihood of getting off the rails, and moreover the less will be the proportionate dead weight of the vehicles to the available load it carries, if it be rightfully constructed. The Americans and other people use long carriages with swivelling trucks supported on a centre pin, which also have eight wheels, and thus radiate the axles imperfectly, though, if the trucks have not length enough, say two feet more than the width of the gauge between each pair of axles, they will not run steady, but will drag their wheels. If the radial system be applied, it would be quite possible to make carriages forty feet long with eight wheels to roll round a curve of 60 to 80 feet radius. A rough model of a vehicle of this description is on the table, and the guiding apparatus of the wheels is not intended for a bogy carrying the load, as in the American carriages, but simply a radial guide, the load of the upper frames being borne by the springs on the axle-boxes, with facility for elastic sliding, or swinging on long verticle shackles.

Some time back a series of medical papers appeared in the *Lancet*, on the subject of the injury experienced by a certain class of patients from railway traveling. The fact was strongly denied by railway authorities, and by many persons in good health who were not authorities. Not long back a railway engineer who had been very doubtful of the injury, informed the writer that he had changed his opinion, for, being out of health, and desiring to travel backwards and forwards to Brighton, he found he could not do it on account of the injury caused by the vibration.

It may be remembered, that when the Brighton line was first opened, numbers of City stock-brokers and merchants took houses at Brighton, and yearly tickets to travel up and down daily, in short, to live at Brighton, and transact their business daily in London. The writer has been informed that many ceased the daily practice at the end of the month, and at the end of six months it was found that hardly any could stand it and preserve their health.

Now, what is the reason of this? The carriages were as comforta

ble and easy, and as well ventilated as an ordinary sitting room when not in motion, and the only difference therefore could be, that the sitting room is stationary, and the carriage moves. But what is the kind of movement? There must be something peculiar in it for physicians to order their nervous patients to travel by road and not by rail. There are two mechanical differences. The road carriage has wheels proper with independent movement and elastic wooden spokes, and elastic springs, and the wheels roll over a rough but not constantly hard surface. Riding in an omnibus along Cheapside, the rider finds the stone pavement hard and irregular, and the iron pavement much harder though regular, and the iron is the most unpleasant of the two.

The rail carriage has a kind of iron garden rollers for wheels, and they run on a small but hard iron surface. If the carriage be traveling at a mile an hour, as when starting from a station, the movement is scarcely perceptible, but when at thirty to forty miles an hour, the vibration becomes unpleasant to most, painful to many. In slow movement the wheels can adjust themselves to the rails, and roll or slide easily. In rapid movement they have no time to adjust themselves, but slide, as well as roll with incessant jerking. On very sharp curves the movement is sometimes all sliding.

It is this sliding which constitutes the difference. We may illustrate it as follows. Everybody knows that the sound of a violin is induced by rubbing the horse hair string of a bow over the strings of the violin, both being in tension. But the simple horse hair will not produce the effect. To produce sound the player applies powdered resin to the horse hair to induce friction, and it is the leaping of these particles over the strings that induces the vibration resulting in "sweet noise." Sometimes, those who love loud laughter better than sweet noise, will, as a practical joke, apply a tallow candle surreptitiously to the horse hair bow instead of resin, and then the vibration causing the "sweet noise" is stilled. Now the wheels on a rail are a contact of practically rough surface, which vibrate and induce torsion of the axle, and thus vibrate and jump, and the result is, not "sweet noise," but very unpleasant noise and jarring, which, if long enough continued, makes a nervous passenger ill, and tends more or less to counteract the peristaltic motion of the intestinal canal. If the rails and tires were rubbed with the tallow candle before alluded to, the vibration would cease, at least till sand enough had accumulated to counteract the effect of the tallow. But the engine-driver would not approve of this plan, as it would lessen the power of haulage.

How, then, is this vibration to be lessened? First, by lessening the hardness of the rail, and rendering it elastic. Secondly, by rendering the wheels elastic. Thirdly, by permitting wheels, or tires, or both, to revolve independently of each other. Fourthly, by radiating the axles so that neither on curves nor straight lines will the wheel flanges be ground against the rails. Fifthly, by efficiently springing the carriages, using a double series of springs, as well known in private carriages. Sixthly, interposing a non-vibrating material, such as india-rubber, between the carriage body and frame. Seventhly, by so

constructing the bodies that passengers may stand or sit at pleasure. The blood cannot circulate freely when in a constant sitting position. Sedentary employment is a common source of paralysis.

The next question is that of brakes in absorbing momentum by friction. When a large mass of material is put in motion it requires at first a much greater amount of power to start it into motion than it does to keep up that motion when speed is attained. The power required to get up the speed is called momentum, and if it be required to stop the momentum it must be absorbed by friction or gravity. If the stopping places of railways were always on ascents there would be no need for brakes; gravity would supply the place of friction. And if the starting places were always on descents, little surplus power would be needed to produce momentum, as gravitation would furnish it. But momentum has to be absorbed under many varying circumstances,—sudden obstacles to be averted or stopped short of,—stoppage at stations,—and the descent of long inclines; also the ascent of long inclines, in case of couplings breaking or wheels slipping.

In the early times of tramways the Convoy (*convoi*) or brake carriage was used with the train. In the early times of railways brakes were applied to first-class carriages, each worked by a guard, because the first-class were the heaviest. But people who paid first-class fares demurred to this, and sliding brakes were adopted, working on the wheels and axles without attachment to the body, and thus jar was lessened but noise was increased, and so the brakes were transferred to the second class, and then the old Convoy—the brake van—was revived, one at the head of the train next the tender, and the other at the tail, and when the train was heavy, one in the middle. But it was to a very small portion of the total weight of the train that the break power was applied, and the guards screwed down the blocks tight, and so skidded the wheels and ground flat places on the tires; and to apply brakes rightly every wheel in the train should have a brake block applied lightly to it, from the engine to the last carriage. The engine should have brakes pressing on both rails and wheels, applied by the driver, through the agency of steam friction, by simply turning a steam cock, the pressure being divided between the rails and wheels in any convenient proportion. Brakes on every vehicle in the train and on all the wheels would reduce the pressure required on each to a minimum. The system of "continuous brakes," as they are called, is applied to as many vehicles as the power used can reach. But the power used is commonly hand-power, and that is limited.

A new mode has lately been adopted on the North London line of making the momentum destroy itself. On this line of short mileage and many stations, with frequent trains, it would not be possible to work the traffic without the means of rapid stoppage, *i. e.*, the utmost rapidity short of concussion. For this purpose a chain of sufficient strength to draw the whole train is extended beneath and throughout the train. When this chain is drawn straight, the blocks are all on to every wheel, and when the chain sinks into a succession of curves, or bights, by a weight beneath each carriage, the brakes are all off.

To draw the chain straight, a pair of heavy cast iron disk wheels, with a barrel or drum on their axle, are then suspended in slings below the last carriage. The guard, by means of a screw, brings these suspended wheels into close contact with the running wheels of the carriage, and the friction causes the disk wheels to revolve by the momentum and to wind up the continuous chain on the barrel till it is drawn straight, and the brakes are all on, when the disk wheels begin to slip, and the momentum of the train is absorbed by the brakes. In this mode a train of fifty vehicles may be stopped in as short a space as a single vehicle, the right amount of friction being applied to each. This arrangement, however, requires either that the chain be carried through the whole train, and accurately connected with the proper lengths of chain to each carriage, the whole train being pulled close together, or that the break-van at one end and the engine at the other, serve as fixed points to lift the counterbalance weights and draw the chain to a straight line. There is no doubt of this acting well on trains fitted for the purpose, but the writer has not yet seen it applied to mixed and irregularly made up trains.

With regard to the application of brake blocks, there is an advantageous and a disadvantageous method. If applied to the fronts of the wheels, that is in the direction of the pulling engine, they do not chatter on the wheels, whether fast, or loosely applied, but when brought into contact with the backs of the wheels they do chatter, till the pressure becomes very strong, so strong indeed as to skid the wheel. The longer the block, *i. e.*, the longer the surface of tire it embraces, the less is the chatter. But it would be far better to arrange the blocks between adjoining wheels, so as to press both rail and wheels, and in this mode there would be no chatter.

But though the continuous brakes before described act so well on continuous trains of vehicles, the action depends wholly on the continuity of the chain, as well as quick movement to apply them. If the chain separates, the brake action ceases at once, and this on a steep gradient might be disadvantageous. It is, therefore, desirable to have the brakes self-acting, if possible, without depending on the human hand, and operating on every vehicle independently of the others—operating also instantaneously when required. The writer was led to study this question, owing to the necessity of enabling the brakes to follow the wheels of radial carriages on curved courses.

To put this in practice brake blocks are suspended from cross shafts attached to frames resting on the axle-boxes of the wheels. To these blocks long weighted levers are fixed, which cause the blocks to press against the wheels with either a steelyard action or, what is called, an elbow joint, four blocks being used, one to each wheel, with two levers or rods to cross bars. Thus the normal condition of the blocks is, to be pressing on the wheels with a power sufficient for the purpose. The lever ends are attached to the brakes rod by a chain passing between two pulleys on the carriage frame, and as in the process of traction the rod moves lengthlong in the carriage to either end, the chain lifts the levers vertically, the traction force of each vehicle

being sufficient for this purpose, and this process operates on every vehicle so fitted, even though ordinary vehicles be interspersed in the train; and in ascending an incline, if a traction coupling parts or breaks, these self-acting brakes will instantly press on the wheels before back movement of the wheels can commence; and in going down hill the brakes will be lifted while the engine is pulling, but will be in action directly the engine is slowed, and they may thus go on pressing and lifting, and so moderating the speed down an incline. It may be objected that the power of the brakes might thus be in excess, and so be a disadvantage, but to equalize this, every vehicle is to be provided with a hand lever, which will enable the driver to put temporarily out of action as many brakes as he may choose, leaving in action the number required for his purpose. The model of the carriage on the table is on a length of rail which may be placed on the level, or at angles of one in 1, in 20, 60, 50, 40, 30, 20, and 10. It will be found that brakes so applied, will retain the vehicle on an incline of one in fifteen, and by snatching the brakes rapidly off and on, starting and stopping may be rapidly performed. In practice the ends of the levers may have sliding balance weights applied, so as to increase or diminish the pressure by the length of the leverage. It must be borne in mind, that the tractive force required to draw the wagon while running, must govern the weight required to be lifted at the brake levers, and in cases where much weight is needed, the mechanism for lifting must be arranged accordingly. When the whole of the wheels are provided with brake-blocks, a comparatively slight pressure on each tire will suffice.

The longer the vehicle the longer may the brake lever be, and the more effective will the vehicles be both for goods and passengers. Coal wagons are made short, for the convenience of getting close to the pit's mouth, but the model on the table, representing a wagon 30 feet in length, will roll round curves of 50 feet radius, and by its great steadiness will carry the coal—a very friable substance, with far less breakage. And a long wagon may be made considerably lighter in proportionate dead weight than two short ones, thus adding to the available load. In these days of competition in coal transit, this is a very important consideration. The self-adjusting brakes described will work equally well, whether the engines be on straight lines or sharp curves as the levers are arranged to act equally well in either case. And as it is needful occasionally to back the train, for which purpose the brakes must be out of action, this may be accomplished by connecting the traction rod to the buffer rods or buffers by spring agency, so that the thrust of the engine may lift the chains and brake levers in succession, and put the whole out of action, or they may be lifted by hand levers as at present practised. This class of brakes is especially adapted to steep gradients. There is nothing new in the principle now proposed. It is the safety principle in the cages of mining shafts and on some railways with rope traction.

The last question we have to deal with is what we may call the police of railway trains. This is a question of structure. Trains are

subject to catch fire on the roof; passengers may be taken suddenly ill; murders may be taking place, or other violence, but the sound of the train deadens all other sounds, and neither driver nor guard knows of it. And so there is an outcry for communication between passenger and guard, and guard and driver. And there are propositions for galleries outside the carriages for guards to walk along and catch murderers in the act. It seems to be forgotten that this facility for the guard would also be a facility for the thief or murderer. The practicability of these things is not considered. It is common to talk of the "six-foot" between the two lines of rails, as giving ample space for the guard's gymnastics, forgetting that the "six-foot" is reduced to two feet by the overhanging carriages, and that two guards on opposite lines would thus come into collision, and, in American phrase, be "rubbed out."

If the guard is to patrol the train, there is but one way of doing it —through the inside—a passage-way through the whole length of the train. Third class would probably not object to this, as they are disposed to be gregarious, and would not object to a guard or policeman additional, but second class might object, and first class assuredly would, on the score that they had paid their money for space and privacy. If it were put to the vote there is little doubt that the vote would be for privacy and a certain amount of risk, rather than gregariousness without risk. It must have been a matter of observation to those witnessing the starting of trains, how first class passengers walk along till they find an empty compartment, and, failing in this, make for one with the fewest occupants. And second class will do the same.

Were the lines single there would be no difficulty, as the carriages might be widened at least to the full extent of the tunnels, but with double lines the "six-foot" is the limit, and sometimes the "six-foot" is only five feet, and even less. With the six feet it would be practicable to have carriages two feet wider, provided only that the passengers would consent to have a wire guard to the windows limiting the protrusion of their hands, like the grating to the windows of a Spanish house. With a carriage ten feet wide, it would be quite practicable to have a closed passage in the centre, two feet wide, and compartments for four persons each, thus getting rid of the objectionable centre seats. The external doors would be retained, there would be sliding doors inside opening into the central passage, communicating with end platforms. The second class and third would dispense with the closed passage. Sliding doors, glazed, would prevent the entrance of wind or rain through the central avenues. In this mode it would not be merely on the guard that the passengers would rely on for protection, but also on the intercommunication with each other in case of necessity. But if the brakers were all left to the driver, as they should be, the guard would have ample leisure to attend to the police of his train, instead of having his time taken up with the mechanism. Great Western carriages in the first class are divided lengthlong by a partition with the disadvantage that the compartment next the platform

on the near side has to serve as an ante-room to the other. If the carriages were two feet wider this difficulty might be avoided by a central passage. No doubt this might be all very much simplified by making the whole train a series of open saloons, as in America, but public taste must decide this; and even in America they are now beginning the system of private compartments. With the exception of occasional alterations at platforms, it would be quite a practicable thing to widen the whole of the existing carriage bodies, by dividing them in the centre longitudinally and inserting the extra width as ships are occasionally dealt with to lengthen them. And there would be one considerable advantage, that by widening the train the length might be shortened, or a larger number might be carried. It may be objected that a small number—four persons—in one compartment might involve a risk in plotted crime; but it would be quite practicable to make one side of the vehicle closed and the other open compartments.

As a matter of safety in collision, the larger carriages are preferable, as they do not mount on each other's backs. As regards the expenditure of momentum by sudden stoppage, which throws passengers violently in each other's faces, the safe remedy would be setting sideways instead of fore and aft; they would be shoulder to shoulder, and in the safest position; but even in this case it is possible that they would prefer the ordinary position with risk, to side-sitting without risk, at least till they had a practical verification by frontal damage to their persons. The subject is so large that the writer has only dealt with it summarily, but a useful purpose will have been answered should the paper have given new materials for thinking.

In conclusion it will be well to help the memory of the reader by a summary of the various proposition contained in the two papers having reference to the amendment of corresponding defects:—

1. Spring tiers for the purpose of preventing blows and friction and lessening the chances of the wheels escaping from the rails, a system thoroughly verified in practice, as quadrupling the durability.

2. Systems of an elastic permanent railway, supporting the rails by continuous elastic bearing, preventing disintegration of the rails as verified in practice.

3. Improved system of fish-joints, duplicating the strength, with elastic fit of ribs and bolts.

4. Radial axle boxes, to enable locomotives of great length to pass round any sharp curve without friction on their flanges.

5. Eight-wheel tank engines for sharp curves of 99 feet radius, long verified in practice.

6. Eight-wheel tank engines, for curves of 99 feet radius, all eight wheels drivers, both on straight lines and curves, by means of super-improved friction-wheels, all capable of retardation by steam brakes, making the whole weight of the engine available for traction or retardation.

7. System of V flanges to spring wires working into V grooves of rails, to obtain increased bite for ascending steep inclines, or when starting with heavy trains.

8. System of long wagons and carriages on eight wheels, radial axles, and guided by quadrants, with sliding springs, or long swinging shackles, to roll truly round curves of from forty to eighty feet radius, and provided with self-acting brakes, to prevent accidents on steep inclines—economically and easily carrying large loads.

9. Mode of constructing passenger carriages to give free internal circulation to guard, engine-driver, and passengers without interfering with the privacy of first class passengers.

These principals and plans are applicable to all gauges, from 7 ft. to 3 ft. 6 inches light lines.

(To be Continued.)

On the Mechanical conditions of Railway working to prevent Destructive Wear and Risk. By W. Bridges Adams.

From the Journal of the Society of Arts, No. 628.

This is a subject not of mere local or of class interest. In days gone by, when a man was by the mass of Cockneys esteemed a traveler if he had been to Gravesend and back, it would have interested but a very few to discourse on the means of transit, but in the present day, all men, women, and children have become locomotists, getting back to a state of nature, like the birds and wild animals, and no longer confined to the spot on which they are born and bred. And a very good thing it is for themselves and general humanity, for they thereby become educated, not in the sense of book learning, but in the sense of knowledge of men and things, expanding their thinking faculties with the more healthy growth of their bodies. We increase thereby the number of men and women, instead of mere human animals, and we increase also national power, which is not to be measured by animal units, but by the mass of intelligence, health, and physical strength—in short vitality. It is better to soar like birds than to burrow like rats; and though we cannot soar above the earth, with guiding power like birds, we contrive to skim along the surface though not quite so fast, and do not yet see our ultimate limit to speed accompanied by safety. We are fast becoming a nation of mechanicians, and each man's strength is as the strength of ten, as fast as he acquires a sound body for a sound mind to dwell in. The subject is possibly a dry one in its details, but out of these dry facts must come the growth of progress, and this must be the writer's apology for dwelling on them, though in as succinct a mode as language will permit. In criticising any subject it is but natural that the critic should devise plans for amending that which he deems wrong, and in so-doing the writer would deprecate any appearance of egotism in dealing with his own plans.

In vehicular transport the contact between the vehicle and the road it moves on may be of three kinds, sliding, sledging, or rolling. The sliding movement may be converted into rolling by the application of water, oil, or unguents, the particles of which form rolling bodies between the vehicle and the road, as with the ship on water, or

with the wine sledges used in Madeira, where the driver sluices the road beneath the sledge runners, or with the winter sledges of cold climates, where frozen surfaces supply rolling particles. If actual contact between the moving vehicle and the road take place without the intervention of rollers or lubricants, destruction must ensue by abrasion.

Where no means exist of supplying lubricants to the road surface, the next process to ease the sledge is to place rollers under it. If the rollers be true cylinders, and are not fixed to the sledge, and the movement be in a straight line, there will be absolute movement without friction. If the rollers be fixed to the sledge, by axles passing through them and bearing the load, the friction will be transferred to the surface of the axle from the surface of the road, and will be greatly reduced if the oil or unguent forms an efficient lubricant. If the roller bears on the road through its length, and be a true cylinder, it will move forward in a straight line. If it be of a conical form it will move in a curved line corresponding to a cone. But if two coned rollers be fixed to the sledge with their axles parallel, the movement will be partly sliding and partly rolling, with great friction in a straight line. If the two coned rollers be not parallel, but are arranged with their axles in converging lines corresponding to the cones, the machine will move in a circle, the centre of which will be the point where the converging lines intersect each other.

The ancient Greek and Roman cars were mounted on rollers, *i. e.*, a pair of wheels connected by a fixed axle running between thole-pins, such as up to the present day we see in the wine-cart of Portugal and parts of Spain, which are unfitted to turn corners, and make so fearful a squeaking when running out of a straight line as to give rise to the jest that they are so ordered to give warning to the custom-house officer in case of smuggling. Double-roller cars also existed, with fixed parallel axles, and this faulty mechanism was probably one cause why the old Roman roads were made in straight lines, as it was easier for such cars to run up hill than to follow curves. Even supposing the case of four-wheeled cars with two wheels running independently on their axles, these cars could still only run in straight lines as long as their axles were rigidly parallel; and in England it was only during the reign of Queen Elizabeth that what were called "turning carriages" first existed, *i. e.*, with a mechanism that permitted the axles to depart from their fixed parallelism to pass round curves.

When railways, as distinguished from tramways, were first commenced, the faulty mechanism of the early classic cars was resorted to as a cheap structure—cheap in first cost. Wheels proper were ignored—*i. e.*, wheels running independently on their axles—and rollers *i. e.*, two wheels fixed to a shaft or axle which revolved with them in thole-pins, or what are now called horn-plates. And, whether two axles or three or more were used, they were always rigidly parallel. And this is the common practice in England to this day.

What are called tramways are formed of flat plates of iron, with rising edges to keep the wheels in track. Ordinary wheels, revolving independently on their axles, were used on them. Their mechanical

disadvantage was that dust and dirt accumulated on the horizontal plates, and the wheels rubbed hard against the rising edges as against kerbstone, and caused much friction. Opinions are divided as to the origin of the term tramway, whether derived from an originator named Outram, or from the word "trammel," to curb or guide. The term "platelayer," on railways, is evidently derived from the original tram-plates. On railways proper, what is called the edge rail is used, in which the increased depth gives greatly increased strength to support the load, and at the same time, dust and dirt have but little chance of remaining on the rails. But to keep the wheels on, the rising ridge or kerb of the tramway is transferred to the wheel, in the form of a flange. It is clear that vehicles with fixed wheels and parallel axles are only adapted to move in straight lines. If the axles were converging, the vehicles would only move in curved lines. But there is another common condition, a result of faulty workmanship—parallel axles not at right angles to the line of traction; in this case the machine becomes a sledge under all conditions, with the wheel flanges constantly grinding against the rails.

With a view to compensate, on curved lines of rails, for the different lengths of pathway on the two rails, it has been a practice to make the wheel peripheries conical instead of cylindrical; *i. e.*, each pair of wheels fixed on the axle being practically a garden roller, with the central portion removed, are made at each end a frustrum of a cone, with the smallest diameters outside. Were the roller solid, *i. e.*, were the coned lines prolonged till they met in the centre, the roller, if balanced, might run in a straight line, or it might run in a curved line, to right or left, if bearing on either cone, the curve being regulated by the angle of the cone. This would be the case with a single roller. But it is a fallacy to suppose that two or more rollers fixed in a frame with their axles permanently parallel, would follow the same conditions, even though sufficient end-play were allowed between the flanges and the rails to make the differing diameters across the breadth of the tires available to compensate for the differing lengths of the rails. The movement in a straight line might be free rolling at low speed, provided the cones had sufficient lateral movement or end-play, but on curves it would only be sliding or sledging. But if the frames were so arranged that the axles were permanently out of parallel by reason of careless workmanship, the machine would be a constant sledge both on straight and curved lines. So also if the axles were perfectly parallel, with the wheels in the same plane, but were not placed at a right angle with the line of traction, the result would be constant sliding friction.

In any of these four conditions—whether a truly constructed frame, with parallel axles, or badly constructed with axles out of parallel, or badly constructed with parallel axles and wheels out of plane, or not at a right angle with the traction rod, though the first may roll at intervals under a favorable condition, the three latter must always slide, and the resistance to traction will be in proportion to the roughness of the rails and the load on them. With a light vehicle, little loaded, smooth wheels and polished tires and rails, and especially if

they were lubricated on the surface, the resistance might be very small. Nay, a sledge might even be better than a wheeled vehicle, if only the condition of a lubricant could be retained. But a lubricant on the rail is inadmissible, first, because it would cause dirt to adhere to the rail, and secondly, it would defeat the condition of adhesion essential to the traction by the locomotive engine. The engine needs the greatest possible amount of adhesion, corresponding to the steam power. The vehicles should be so constructed as to minimize the amount of resistance. But as at present constructed, the vehicles on a railway are in principle a reproduction of the old Roman cars on two rollers, with such variation as a better or worse condition of workmanship or lubricant may induce, and the change from a common road to a railroad.

Axle friction, under the best condition, is commonly estimated at about four pounds per ton of load, but this is usually doubled by a condition, that under the best treatment ought scarcely to exist, viz: "rolling friction" *i. e.*, the friction of the tires on the rail, and thus at a moderate speed on the level, eight pounds per ton is estimated as train resistance. But this, it is well known, is very far short of the real resistance in practice, which by quick curves and bad structure may be quadrupled and quintupled, varying with the speed of movement.

A river running with a slow movement may pass along a winding channel without disturbing its banks; but the same river, with its speed increased by a sudden influx of water, seeks to make a straight course and cuts away its banks, or *rives* them, in a mode corresponding to its etymology. Even so, a railway train at high speed becomes a *river* of the rails, at an increased cost of coke converted into steam; and, like the river water, it produces debris, not in the form of gravel, but of black iron powder, as any one may verify by rubbing his finger along the rails after the passage of a train. This is frictional destruction, increased or diminished, in proportion to the load on each wheel.

But there is yet another element of destruction—percussion or blows which take place between wheel and rail, friction or percussion being the only sources of mechanical destruction on railways—the others being chemical. It is quite clear that were a train to stand still on a railway, and never move, it might rust and rot, but it would not wear out, and though the proverb is a sound one "better rub than rust," it is still better to do neither. The movement of the train begins the great destruction, and it is want of compensation for irregularities—a condition never disregarded by nature—that causes the destruction. The wheels, when running on unequal paths, induce great torsion of the axles by friction on the rails. At bad joints or uneven surfaces the wheels jump from the rails, and they then recover their normal position, by the axle as it were unwinding itself like a discharged spring, and striking a violent blow on the rail. Sometimes the wheels, with the axles in a state of torsion, drag along for a considerable distance, heavily loaded, and this is a fruitful source of axle breakage as well as of rail destruction. Again the flanges strike against the rails from side to side, and a constant succession of blows and vibrations is induced throughout the whole of the train. And with long vehicles, on

sharp curves, there is a constant tendency to grind the flanges and burst out the rails. The rapid wear of wheel tires and rails yields ample evidence of this, and the probability is that at high speed the movement is as much sliding as rolling—side as well as forward sliding. But for the partially polished surfaces of both wheels and tires facilitating slip, at the loss of adhesion on the driving wheels, it is probable that the destruction would be much more rapid.

Are there proofs needed of all this? Time was that rails of 60 lbs per yard, and Staffordshire tires too tough in texture to break, were capable of considerable duration. They have gone by, and rails, after various experiments in making them harder in iron, have grown to 84 and 90 lbs per yard, and now with Bessemer steel for metal; and tires, after running through the phases of Park-gate, Low Moor, and Leeds, and various plans for steeling their surfaces, have now culminated in Krupp's solid steel.

It is many years since the writer became aware of the importance of elastic action, even on wood wheels used on highways and paved streets, to induce durability, and facilitate traction, when high speeds were needed. In the structure of the ordinary wood wheel the strength resides in the tyre, which keeps the weak frame together. Originally, wheels were made vertical, *i. e.*, with the spokes all in a plane, and the width of base was determined by the ruts or hollows which pervaded all roads, giving a kind of fixed gauge, much as rails do now. The bodies were placed between the upper part of the wheels; and when it was needed to widen the bodies, the wheels were made in the form of a cone, or what is called "dished," the axle-arms being pitched downwards to keep the lower spokes vertical, with an idea of strength, while the upper spokes inclined outwards with a considerable angle. The fellies and tire were made conical, to preserve a flat tread on the ground. It is evident that as these wheels revolved, they were constantly grinding the road, the outer side of broad wheels being considerably less in diameter than the inner, while the vertical spokes were continually driving into the nave and loosening the framing.

After trying some experiments with wheels in which hoops of elastic steel were used to connect the periphery with the nave, instead of spokes, the writer had some made with the section of the wood spokes reversed. The ordinary mode is to make the spokes deepest from back to front, but in the improved samples were made broad in the plane of the wheel, and thin from front to back. As this was a bad form to fasten in the ordinary nave, it was dispensed with, and a pair of disk wheels were substituted, between which the spokes were mitred together and bolted. One felly was used to each spoke, instead of the ordinary practice of one felly to two spokes, and when cylindrical tires —not conical—were shrunk on, the spokes curved from back to front, and the wheel, instead of being a rigid cone, or dish, became an elastic dome each pair of opposite spokes radiating from the centre, representing the form of an archer's bow. The fastening of the tires was through the joints of the fellies. When applied to a straight axle, with the arms not pitched downwards, the load was carried, not on a

rigid vertical, but on a bent inclined spoke, and the result was that the load was not only carried more easily, but the wheels were, for their scantling, the most durable ever constructed.

In the early railway wheels the principle obtained of neglecting the structure of the frame, and making it depend for strength on the shrinking of the tire, just as wood wheels were made. One result of this was the flattening of the tire between the spokes, and a tire of unequal thickness, when turned in the lathe to make the external periphery a true circle, and a consequent rapid destruction in wear. This applied to the wheels with the wrought iron spokes. The truest formed wheels were those of cast iron with a wrought tire. They might break if of careless moulding, but they would not otherwise alter their form, and the tires could be all of equal thickness. But upon rigid cast iron wheels the tires became exposed to very rapid destruction as the loads increased.

The writer had early turned his attention to the importance of wood wheels for railways, and as far back as the year 1838 caused some to be constructed with two rows of fellies, break-joint between the tire, and a cast centre. They worked well, and the writer has been informed that they still exist. The difference between these wheels and the modern wood wheels is that they had the elastic side grain on the tread, whereas the modern wheels have end grain. The reason for substituting the latter less perfect method probably was the difficulty of ensuring dry timber on a large scale, and wood shrinks scarcely at all endwise, so that the shrinkage involves less evil.

Wood wheels on railways are, as now made, solid disks, and therefore the frame preserves its circular form without being excessively rigid. Iron wheels have also been made in the disk form, both in cast and wrought iron; but, then extreme rigidity rendered the destruction of the tires a very rapid process;—the tire was between two anvils—the frame of the wheel, and the rail supported on chairs of cast iron.

To prevent this destruction the writer early devised a wheel in which the spokes were all springs, but that did not answer, because the bending of the spokes prevented the wheel from running true. The next plan was to apply a hoop of elastic steel between the tire and the periphery of the wheel, with a hollow below the steel. The wheel was retained in the tire by a rising rib in front, solid with the tire, and by a lower rib behind, sprung into a groove of the tire. Models are on the table and a section on the wall.

The plan was satisfactory in theory, but how to put it in practice was another matter. A railway was needed, and railways are not to be found in a private field or park; they are the highroads of the public, and in charge of officers whose first care is to take no risk by an unknown plan; for if an accident happens through a known plan in common use, however faulty in its mechanical philosophy, a jury will acquit the manager of all blame. But if the accident happens with a novel plan, however theoretically right it may be, the jury will probably condemn it as new-fangled, and saddle the manager or engineer with the blame; and thus there is a natural indisposition in engineers

to step beyond their tether into the unknown, and they can hardly be blamed for their caution.

So the writer went forth on his propaganda to expound an unknown quantity, but few would even listen to him, and none would believe; but strong in faith he persevered, and at length found an engineer with a logical mind, in the locomotive superintendent of the North London Railway, Mr. William Adams. The writer demonstrated his principle, and Mr. Adams was unable to disprove it logically whereon the writer remarked; "If you cannot demonstrate it to be wrong, and I demonstrate it to be right, and that the result if successful must be very advantageous, you are bound to try it." He agreed to this, and said "I will try it, though doubting greatly. What wheels will you apply to it?" "Those most destructive to tires."

So a set of wrought iron disk wheels were selected, and Staffordshire tires were applied to them on hoop springs, and they were placed under a heavy carriage. At the same time a set of ordinary spoke wheels, with a set of Low Moor tires fixed in the rigid mode, were applied to a similar carriage. The Low Moor tires required turning up to restore their surface after running less than 50,000 miles. The Staffordshire tires on springs ran 106,000 miles without turning up, and with no alteration of form, and were then taken off to exhibit, and may still be seen in the same condition.

There is no mystery in this. The Low Moor tires were exposed to severe wear. First, because on curved lines they were necessarily forced to slide on the rails, both forward and laterally. Secondly, because they were rigidly fixed, and possessed no elastic yielding to elude blows. Thirdly, because on curved lines there was a constant torsion of the axles causing incessant jumping. Fourthly, because the tires had no power of lateral movement to suit the varying inequalities of the rails. In short there was no compensation for irregular movement, and the result was a constant grinding of the flanges and treads.

With the spring tires, on the contrary, there is universal compensation. First, there is no tendency to blows, because the elastic cushion preserves constant contact between tire and rail. Secondly, there is no torsion of the axle or slipping of the tires on the rail because the wheel can, on curved lines, slip in the tire on the smooth surface of the spring without damage, thus equalizing the pathway. Thirdly, the tire can rock laterally on the wheel to adjust itself to the bearing on the rail surface, which any one may observe to be constantly varying, sometimes the middle of the rail being most prominent, sometimes the inside edge, and sometimes the outside. No other class of wheel can supply these various compensations.

Important as these wheels are for vehicles which are simply drawn, in order to remove resistance, they are still more important for engines with the load greatly increased, and especially for driving wheels—more especially for engines with four coupled drivers. It is obvious that if two wheels of equal diameters be fixed on the same crank shaft they must grind and drag on curved lines, and the torsion thence en-

suing is the main cause of the breakage of crank axles, which it is difficult to make strong enough for their work, the axle being in reality a very long axle, as would be readily apparent were it stretched to a straight line—between eight and nine feet long on the narrow gauge. When four wheels are coupled, the diameters very rapidly alter by irregular movement, and the adhesion may actually become disadvantageous, retarding the engine by friction, like a brake. It is clear therefore that the power of slipping the wheel within the tire to compensate for curves and irregularities becomes a question of the greatest importance, both as to facilitating haulage and preventing the wear of tires.

It has been at times assumed that the slip of the tire on a wheel must defeat the purpose of haulage. But when it is remembered that the bearing of the wheel is over half the diameter by downward pressure, and that with a heavily-loaded wheel a slight flattening of the tire takes place, it is obvious enough that slip will only take place by excessive friction on the rail; this is the true theory, but, for the satisfaction of those who only believe in practical results, it may be stated that the experiment has been tried repeatedly, and it has been found that when the tires were so loose on the springs as to be turned round by hand with the wheel lifted, the haulage was in no way affected. On a London line, of sharp curves, where the leading tires of ordinary engines with parallel axles had their flanges constantly ground off in two months wear, a bogy-engine was set to work with a turning centre to radiate the leading axle to the curves. At the same time a similar sized engine, but with parallel axles, was applied with spring tires to the leading wheels. The result was that in both cases the leading flanges were saved from damage, and ran three times the ordinary distance. The yield of the springs had an effect similar to that of the play of the axle to right and left in the bogy, when truly centred, for if not truly centred the flanges wore unequally.

These spring tires were adopted on the engines of the St. Helen's line, by Mr. James Cross, the engineer. That line is an incessant continuation of sharp curves and gradients, some of the latter 1 in 35, and some of the former two chains radius. The spring tires were first applied to a six-wheeled engine, all coupled drivers of 4 ft. diameter. They were all of Staffordshire iron. At the same time another engine of less weight, also on six wheels but of 4 ft. 6 ins. in diameter, and only four coupled, was fitted with tires of Krupp's steel. After running 41,000 miles, Krupp's tires was so deeply worn as to need turning up, while the Staffordshire iron tires ran 65,000 miles, and then only required the flanges to be reduced which had been deeper at the outset, the tread of the tire remaining in good shape. Taking into consideration the difference of diameter, the distance run by the Staffordshire iron was as 72,000 against Krupp's steel 41,000. In practice, Krupp's tires was found to slip on the rails even in dry weather, but the Staffordshire iron was scarcely ever known to slip, though working steeper gradients and sharper curves.

Another set of experiments was tried with two fellow engines, one

of them fitted with spring tires, the other with rigid tires, up a gradient of 1 in 80, during a whole day, by the same driver. The result was, that the rigid tires constantly drew up 13 loaded coal wagons, and the spring tires fifteen.

The result of this elastic principle having been so unmistakably advantageous in wheels, the writer began to consider whether the same principle might not be practically applicable to rails also. It is clear that the destruction of rails arises from blows between the wheel tires and the rails, increased in destructive effect in proportion to the load, and therefore it is, that it is now proposed by some railway companies to substitute steel for iron in rails as well as tires.

On the North London Railway, the rails are 82 lbs per yard, and their average duration on the main line does not exceed four years, if so much, the traffic on each pair of rails being upwards of 120 trains per diem of goods, coal, and passengers. The great wear is not owing to wear of attrition, but to crushing, disintegration of the laminæ. The rails are double-headed, and worn out on both tables before they are taken out. The sleepers are three feet apart, and the rails are fixed in cast iron chairs with wooden keys, the joints being fished with long fishes and four bolts.

Iron rails are in their manufacture practically "scrap iron." They are formed of bars of various sections piled together and heated, and then rolled out. In the act of heating, the oxygen of the atmosphere gets access to their inner surfaces, and scale is formed. This scale does not roll out, but rolls in, keeping up a constant separation of fibres like the grain of wood. With a certain intensity of blow on the line, the chairs and sleepers serving as anvils, the fibres are crushed apart, and the utility of the rails is destroyed.

Steel rails are not formed of separate masses welded together, but each of a single ingot rolled out without flaw or imperfect junction. They are homogeneous, which the iron is not, and therefore do not disintegrate with the same amount of blow which disintegrates the iron. But to be safe, the steel rail requires to be not only homogeneous in metal, but in the temper of the metal also. If the steel rail be taken hot out of the rolls and thrown on a cold iron or stone floor, it may become partially chilled—hard and soft—and in this condition it is apt to break, for which reason it is always recommended to keep the steel rails as soft as possible, to prevent risk, in fact to reduce them to the condition of iron. But the better plan would be to use spring steel, and harden and temper it.

There is a notion prevailing that permanent way on cross sleepers is elastic by virtue of its discontinuous bearing. But it is obvious that if there be any yield of the rails between the supports, there is none at the chairs themselves. If the rail be loose in the chair, which it commonly is, and if the sleepers be loose in the concreted ballast, which they are commonly the looseness only serves to give momentum to the blows of the wheels, so that the rail drives into the chair, the chair drives into the sleeper, and the sleeper drives into the ballast, while the fish-joint bends at the weakest point.

On a portion of the main line of the North London, the rails and chairs being taken up, longitudinal timbers, four inches deep by eleven inches wide, were spiked down to the sleepers. On these longitudinals, midway between the sleepers, were placed cast iron brackets in which the rails were fixed, suspended by the upper table, and not resting on the lower table, as in chairs, the bottom of the rails being 1¼-inch above the longitudinal timber, so as not to touch it. The joints was fastened as usual. The cross sleepers were packed hard and tight on solid ballast. The longitudinal timbers were not packed, the result being that the rails were continuously supported on an elastic base. With the traffic of the trains the mechanical action was, that no wheel pressed directly on a single sleeper with a hard blow, but that the weight on each wheel was distributed over two or more sleepers, and that through an elastic medium. Practically, after nearly three years wear, none of these rails have been found to disintegrate, but are as perfect as when first laid down, except where connected rigidly to the ordinary line, and there the ends of the rails are split; and the elastic action is not merely vertical, but horizontal also, by a slight twist of the longitudinal timbers preventing side blows from the flanges of the wheels.

It was also found that the sleepers remained quite undisturbed in the ballast, owing to the distribution of the weight, and that they really became *sleepers*, instead of dancing up and down. Moreover, the provision for elastic action dispensed with the practice occasionally resorted to of digging up the ballast below the sleepers to soften it, and prevent the extreme rigidity. It is getting to be a known fact that constant traffic and the use of heavier engines and vehicles is gradually solidifying the whole of the railway lines, so that the destruction of the plant, both fixed and rolling, is on the increase. A superintendent of a long line running on hard ground informed the writer that he contemplated re-spacing the whole of his sleepers in order to use the softer intervals. An illustration of this great evil may be found in the fact that in the winter time when all the ground becomes hard with frost, the destruction of wheel tires and rails by breakage, is greatly on the increase.

After the successful result of the first experiment on elastic lines, the superintendent of permanent way, Mr. Matthews, laid down a second portion, in which the cross-sleepers are six feet apart, or double the ordinary distance, the supports of the rails on the longitudinals being three feet apart. The same result was obtained in preserving the sleepers steady in the ballast, though the elastic yielding was greatly increased, being quite perceptible under the rolling trains.

Ballast in England and elsewhere has become a kind of stereotyped custom on railways, though there are districts, as Egypt,—the Southern states of America—where a driver "guesses he never run on ballast but only on mud roads"—the Pampas of Buenos Ayres, the Llanos of Venezuela, and elsewhere, where no ballast can be had, and so it is dispensed with. But the theory of ballast has hardly been considered. It was originally used on non-porous soils, as a kind of

easy drainage for surface water, and in some places tolerably cheap. And it does not yield like clay. But it very soon ceases to be porous under the sleeper, and becomes a kind of conglomerate, or concrete, each sleeper pit holding water like a pond. But inasmuch as it has been practically demonstrated that with a provision for elasticity the sleepers may advantageously be rigid fixtures, there seems to be no reason why the whole surface of a railway should be covered with 18 inches of gravel or broken stone, varying in price from 1s. to 2s. 6d. per cubic yard. The object should be rather to cover it with non-porous material, so as to keep out the surface water, the same principle on which we pave the footways of our streets; and there is no doubt that it would be cheaper in many cases to use stone flags or cast iron for sleepers, rigidly fixed and overlaid with elastic longitudinals, so as to dispense with ballast, even in first cost, say nothing of the saving in maintenance. As regards such lines as are constructed on brick arches, the elastic system would prevent the vibration which so commonly disintegrates them, and, by ramming down the ballast, forces out the parapets; and it would be better to cover the surface with non-porous material, such as paving stones, and get rid of mud and dust altogether. The action of this novel system depends upon the elasticity of the longitudinal timber, but inasmuch as there are circumstances in which timber is inadmissible, such as very hot climates, or climates of great heat and moisture combined or alternating, the writer was led to consider whether it might not be practicable to obtain the elasticity by the agency of metal. This can be done in two methods, substituting a broad T-iron for the longitudinal timber, or by applying springs of tempered steel beneath the seats of the rails, which in that case cannot be suspended by the upper table, but must rest below the vertical web, being supported between the bracket sides with provision for the small amount of movement required. Two models of cast sleepers are on the table, with elastic steel bases. The cost of them will not exceed ordinary iron way, as great weight of metal is saved.

It is desirable that the rails should be of such depth as not to deflect beneath the wheels, and also that they should not deflect laterally, for which reason the tables should be of ample width. It is also important that the rails should be so joined together as to form non-deflecting bars at the joints, though with provision for expansion and contraction. In examining the ordinary fishes it will be perceived that the original principles of the writer's invention have not been carried into effect. These fishes as ordinarily used, are parallel bars, about eighteen inches in length, and with four bolts to attach them to the rails. As the total depth of the fish is only between two-and-a-half to three inches, it is obvious that a pair of three inch bars cannot possibly be equivalent in strength to a rail five inches in depth, and provided with two broad tables. In order to compensate for this defect in some measure, the fishes are made as thick as possible, approaching an inch, making them very rigid. They are wedge-formed between the rail-tables, but at an obtuse angle, not an angle of repose, and they depend wholly on the bolts to keep them in position under

the action of the trains, and the slightest movement of the rail loosens them. For this reason the bolts are made as large as possible. The large hole weakens the fishes, and it is necessary to drill the holes, as punching distorts a narrow thick bar. Moreover, as the fish is as thick at the ends as at the centre, the result is a blow from the wheel at the fish end, where it is too strong, and a second blow at the joint where it is too weak, and a gradual dent at the fish end into the rail, and another dent of the rail-end into the fish; and when these dents begin to form, the process of destruction is certain; rust is formed, rubbed off, and begins again, and the noise and jolt is rendered very perceptible to the passengers before the cause is detected by the eye, except when a train is passing, when the deflection is very perceptible.

To remedy this evil, the writer has devised a true form of fish. It is tapered from the ends towards the middle, where it is the full depth of the rail on the outside fish, and on the inner the full depth of the rail, less the upper table to make room for the wheel flanges. Angular ribs are rolled on the inside of the fishes, to fit accurately against the rail tables, and the fishes are arched laterally. The metal being thin, except at the ribs, they are easily punched without distortion, and when the bolts are screwed tight, the arches of the fishes flatten and press the ribs against the rail tables firmly. The bolts thus acting on elastic surfaces, the nuts remain tight, and the joint is elastically firm. The models of the elastic way, both in timber and iron, with steel springs, as well as the improved fish-joint, are on the table.

The writer has dwelt upon this subject the more earnestly because on any new plan connected with a railway, a protracted experiment is needed for verification, and this protracted experiment of elastic railway has been made, and can be examined by all who feel an interest in so important a subject.

(To be continued.)

For the Journal of the Franklin Institute.

Discussion of some Trussed Girders of one Arched Chord.

By S. W. Robinson, C. E.

This article, I intend to make a discussion—founded principally upon numerical results—of the economical arrangement of material in building trussed girders with one straight and one curved chord or stringer.

As iron comes into use for the construction of public works, it is very important, especially at present prices, to make that disposition of the material which shall result in securing the object sought with the least possible amount, for numerous reasons:—first and primary, *cost.*

There is no case where the cost of a structure can profitably be lost sight of by its projector. All modern bridges could have been built in the tubular form, of iron: a form possessing a desirable degree of

rigidity and durability. The Niagara Suspension Bridge has established, beyond all doubt, the success of that form of bridging for both carriage- and rail-ways, though the construction of a tubular bridge in its place was a possibility. But the prospect of reducing cost ruled in the adoption of that form. And so in every case.

Of two or more methods of construction which will give equal satisfaction in every respect at varying costs, the minimum, if known, will prudently be chosen; and anything pointing this out should receive careful attention.

In the trussed girder of one bent chord, some of the principal points to be considered in respect to this are the depth of the truss near the middle compared with its length: the inclination of the tie rods in the panel trussing: and the form of the curve adopted for the arched chord.

The example I have selected for the basis of my numerical results is the same throughout, unless otherwise designated.

Let $2\,\text{B}$ = span = 160 feet.

a = depth of truss = 20 feet.

i = the angle of the tangent line to the curve of the arched chord, with the straight one, or with the axis X.

The loading one ton per foot along the roadway.

And of the two diagonals of a panel, let θ be the angle, reckoned from the straight chord, for that inclined toward the centre, θ^1 of that inclined from the centre; x and y co-ordinates to the curve of the chord.

The equation of the curve for the parabolic arc, origin at the crown, is

$$x^2 = \frac{\text{B}^2}{a}\,y. \quad \text{We also have} \quad \frac{dy}{dx} = \frac{2\,a\,x}{\text{B}^2} = \text{tang } i,$$

$$y = \frac{\text{B}^2}{4\,a}\,\text{tang}^2 i, \quad \text{and tang } i = \frac{2\,a}{\text{B}}$$

making $x = \text{B}$, the point of junction of the chords; then for the given quantities,

$$\text{tang } i = \frac{40}{80} = 0{\cdot}5. \quad \text{Hence, } i^1 = 26^\circ\ 33'\ 54''.$$

Of the Depth of Truss.

Now, if we assume the weight of a cubic foot of the iron framework equal 450 pounds, and the weight of the diagonals, &c., equal $\frac{1}{8}$th of the arched chord, whose transverse section is K, and make the strains depend upon the weight of the frame together with that of the loading of 2000 pounds per foot in length of the truss; and take R = 20,000 pounds for the resistance of the iron per square inch of section; then I find for the section K,

for $a =$	26 feet.	K,	26·0 square inches.
" " "	35 "	"	19·6 " "
" " "	49 "	"	13·0 " "
" $a = B =$	80 "	"	8·3 " "

These figures show that the strains and the depth of the truss are nearly in an inverse proportion. But there is a limit in every practical case beyond which we should not attempt to pass. The change in the strains of the tie rods, &c., is small, compared with other changes by increasing the depth.

Of the system of inclined tie rods for the panel trussing of girders of one arched chord.

For an example, take the quantities given above, and let one of the curves adopted be the arc of a parabola of which the equation is also given: and $i = 26° 33' 54''$. Let there be 11 normals dividing the straight stringer into 12 parts, making 10 panels and 2 half panels. Divide i into 6 equal parts, and let the intersections of the normals with the curve be at points where the angle of the tangent to the curve is a multiple of $\frac{1}{6}i$. This causes the spaces to increase from the centre toward either end, but will answer for the present case. The strains I have computed are about the fourth panel from the middle, or third bay of the horizontal cord from the end.

I get $x_3 = 37·77$, $y_3 = 4·458$,
$x_4 = 51·09$, $y_4 = 8·158$,
$i_3 = 13° 16' 57''$, $i_4 = 17° 42' 36''$.

For the angle of the arched chord between the points $(x_3\ y_3)$ and $(x_4\ y_4)$.

$$\text{cotan } i' = \frac{x_4 - x_3}{y_4 - y_3} \quad \text{from which} \quad i' = 15° 31',$$
$$\text{and } \theta = 58° 28', \quad \theta' = 34° 53'.$$

For this panel the maximum stress of the arched chord, the truss loaded throughout and the diagonals acting as braces, is 165·2 tons. At this instant the strain of the brace of this panel is 6·0 tons.

Where the diagonals act as ties, the stress of the chord is 169·3 tons; and of the tie 5·4 tons.

For the *maximum* of the diagonal, the load extending from the remote end to this panel only; as a brace, is 15·9 tons. As a tie, 18·3 tons.

Now, if the normal tie rods be made vertical by swinging their lower ends, the maximum stress of the bent chord for this panel is 166·1 tons. The maximum of a diagonal as a brace is 20·5 tons; as a tie, 17·8 tons: and for a load throughout the truss the diagonals are relieved altogether.

The strains produced upon the diagonals when the loading extends throughout the whole structure and the tie rods normal, are compression upon those inclined from the centre, and tension upon those in-

clined towards it; while those which receive the maximum are the converse; that is, the maximum strains produced when the loading extends to the remote end; but the same when the loading extends to the near end, and this probably exceeds it.

The above figures show that no economy follows from inclining the tie rods when the diagonals act as ties, unless from the diminished stress of the straight chord, which at this panel is about 160 tons. But they perhaps show a slight gain when the diagonals act as braces, though the strain of the lower chord is then about 163 tons at this panel. This slight difference in our favor is mostly by reason of the thrust or pull of the diagonals when the bent chord has its maximum, which in one case is diminished and in the other increased. By this we would infer that if we adopt for the curve of the arched chord one of equilibrium for normal tie rods, or that which will cause the strains upon the diagonals to become zero when the maximum of the cord occurs, or when the load overreaches the whole truss, we may effect an advantage for the use of either ties or braces.

Curve of Equilibrium for Normal Tie Rods.

The new curve of equilibrium which I have investigated and given in the March number of the Journal for 1864, will be found to correspond exactly with the proposed curve.

The radius of curvature of this curve has its maximum* where that of the parabola is at its minimum, viz: at the apex or at the point corresponding to the crown of the arched chord. Also, for the parabola tang $i = \frac{1}{2}$, and in the equilibrated arc sin $i = \frac{1}{2}$ where the chords meet.

Now, if the origin and junction of the chords for both cases are at the same points, it is plain that the curve of equilibrium must lie entirely outside of the parabola between the points mentioned, the maximum departure, which, for the given example, is ·64 feet, being where a normal to each curve will become coincident with each other.

From this it follows, that for the new form we will have a greater depth of truss throughout except at the middle point. Therefore, we would naturally predict for this curve a diminished stress each way from the middle.

The properties of this curve, and its application to bridges, are discussed at some length in previous numbers of this Journal, where it is found that the stress is uniform and equal to that of the parabolic arc at the apex, or of any other curve where a and B are constant for all. In the June number for 1864 I gave the strains of the chords and diagonals for the same example as above. For the arched chord it was found to be 160 tons, and 160 tons at the middle of the straight stringer, which is less toward either end.

Among the results exhibited above we find none more favorable than those pertaining to the arc of equilibrium. Thus, to recapitulate, we have for the parabolic arc under the three conditions, 165·2, 169·3, and 166·1, while for the curve of equilibrium we have 160, or

* See March number for 1864.

a saving of 5·2, 9·3, or 6·1 tons, and this at the fourth panel only, from the middle. These differences are still greater at the junction of the chords.

Other Curves for the Arched Chord.

I have already observed that the radius of curvature increases for one, and diminishes for the other curve in passing from the origin. If, then, we adopt the curve having a constant radius of curvature, (the circle,) and the normal tie rods, we would approximate to the true form of curve, the curve of equilibrium. The circle, therefore, possesses advantages over the parabola as a form of curve for arched truss girders with normal tie rods.

The ellipse probably approximates still more closely to the true curve.

But for this, as well as for the circle, and even the parabola, when normals are introduced, the equations and formulas necessary to get the dimensions, and stresses of the parts, are much more complex than those for the arc of equilibrium, excepting, perhaps, the equations of the curves themselves.

For the Journal of the Franklin Institute.

Papers on Hydraulic Engineering. By SAMUEL MCELROY, C. E.

No. 5.—*Fire Service and Hydrants.*

(Continued from page 92.)

Defects of Distribution.—Assuming entire adequacy in the fire department of a city, it is evident that the conditions of fire service, aside from the engines, require a proper reservoir head, distribution service, and hydrant arrangement, and cannot be otherwise correctly fulfilled.

In this, however, we except the process of engine supply by *cisterns*, to which some allusion is important in this connexion.

In cities which have grown to large proportions before the introduction of the Constant High Service, and depended on wells and pumps, it has been customary to construct large cisterns in the streets, for engine use. These have been supplied by rain-water, or by hand pumping from adjacent wells, and in some cases, after the introduction of water-mains, by special supply pipes. There are few large towns without these cisterns, some superseded and out of use, and others maintained for service.

In some cases, as at Boston, the deficiency in water head and hydrant calibre has led to their careful construction and maintenance as an important reserve. The Boston Report for 1864 refers to 15 such cisterns, which had been connected with the main pipes, making 96 in all; the Cleveland Report of April, 1862, refers to 49 cisterns, of which 27 had been connected with the mains; the Detroit Report for 1864 refers to 122 cisterns supplied by 3-inch valves and 5 by

4-inch valves, the necessity for increase in size being explained by the Report for 1861, which says:

"Since the introduction of steam fire engines into the city, complaints have been made of the insufficiency of the supply of water in the street reservoirs in some localities; this may be remedied by introducing an additional stream into them."

The Report for 1854, p. 25, thus details the operations of these reservoirs:

"In addition to the fire plugs, there are now sixty-five street reservoirs. * * * Twenty-one of these are supplied with water by a three-inch pipe, terminated with a valve. Early in the coming season, I hope to have them all supplied in the same manner. The old method of supplying them with wooden logs, terminated with plugs, was very insufficient, and caused a great deal of trouble and complaint. Upon examination, many of the logs are found to be connected with lead pipes of one inch diameter, and even some with pipes of five-eighths of an inch diameter. The logs in no case exceed a diameter of two inches. The consequence is, that two fire engines, worked constantly, will exhaust one of the reservoirs in from twenty to forty minutes, notwithstanding the plug is out and the water running. * * There is a further advantage in the use of a valve at the end of the pipe. The plug being almost invariably kicked into the reservoir on the occasion of its use, it is not likely to be recovered, and another is required to be furnished; and whenever it is replaced by driving it against the head of water, the operator is pretty sure of a good wetting. The valve being raised and lowered by means of a key, will prevent these inconveniences."

Without multiplying these instances, we may refer to a case, where no hydrants are in use. The Louisville Report for 1861 says:

"One hundred of the street cisterns for the Fire Department are connected with the water pipes. No fire-plugs are in use, their efficacy for supplying steam fire engines with water has heretofore been regarded as doubtful. In some of the Eastern cities they are, however, used in addition to street cisterns, and regarded as equally efficacious, and much more economical in construction and the use of water, from the fact that cisterns, as generally constructed, are frequently leaky, and particularly so in streets where there is much heavy traffic, subjecting the cisterns to violent jarring and consequent leaks. It is believed that fire-plugs, properly constructed, can be introduced with advantage to the steam fire engines and great economy in the use of water, especially along the 6, 8, and 10-inch lines of pipe."

While these extracts embody a sufficient condemnation of the class of fire-hydrants in common use, as entirely insufficient for their special work, they also embody a sufficient summary of objections to the use of cisterns, viz: a cost which limits them to wide intervals of position and prevents their service for large fires; a limited capacity while in service; a want of durability; and an additional load on fire-engine pumps by the necessary suction lift, as contrasted with the relief of reservoir head, through a proper hydrant connexion; they are also out of question for the first process of fire-service, which is independent of engines.

In connexion with the subject of distribution, the following table is given, as embodying some of its leading characteristics in practice.

Distribution Statistics.

Locality.	Year.	Population 1860.	Houses supplied.	Reservoir Level.	Distributing Mains.	Fire Hydrants.			Stop-cocks.	Public Cisterns.
					Miles.	Intervals.	Number.	Repaired.		
Albany,	1863	62,368		156·245	49·36	feet. 976	267		167	
Baltimore,	1861	212,419	16,915	150	136·00	1253	573	74	656	
Boston,	1861	177,481	24,720	123·5	131·30		1451	199	1188	24
	1864				136·60	471	1530	94	1312	96
Bridgeport,	1853			115	15·00		20			
Brooklyn,	1863	266,664	taps. 17,145	172·197	164·53	711	1220	378	968	
Buffalo,	1859	81,131		88	32·00					
Chicago,	1862	109,263		85	95·30		469	55	221	
	1864				117·90	1135	548		298	
Cincinnati,	1862	161,044	taps. 13,333	175	88·40					
Cleveland,	1861–2	36,054		160	24·38		142		285	49
	1863–4				27·32	912	158		325	48
Detroit,	1862	45,619	7,275	77·5	68·00		240	17	295	124
	1864				72·60	1539	249	12	312	127
Elizabeth,	1858			00	7·00		50			
Hartford,	1862	29,152		121	29·08	1030	149		180	
Jersey City,	1861–2	29,226		124·5	39·65	656	315		262	
Louisville,	1863	69,470	1,211	150	32·26		none.			124
New Britain,	1859			175	10·00		16			
New York,	1863	814,287	60,000	114	290·48	586	2615		ab't 2200	
Philadelphia,	1862	568,034	76,299	98·124	346·12		3055		3779	
	1864				363·80	538	3255		3967	
Washington,	1863	61,122		145	17·35					

Reservoirs under the Constant High Service, or their equivalent stand-pipes, are rarely open to the objection of an insufficient head to control the highest buildings of their district. Care is generally taken in this direction by hydraulic engineers, and reservoir sites are determined with regard to the highest planes of the supply district. In many cases the head is excessive for the great majority of buildings supplied, being arranged for the use of smaller and more elevated planes, and instances occur of two or more reservoirs at different levels so connected in their district supply mains, as to make the highest available for the lower levels.

In London, with an Intermittent Service, the hydraulic heads range from 82 to 120, 157, 185, 207·5, 350 and 430 feet above Trinity datum, 120 feet being sufficient for the city proper; at Glasgow the Gorbals Gravitation head is 225; the Kilmarnock 270, and the Stirling 450 feet; the heads at Detroit and Fairmount are low, 77·5 in the first case and 98 in the second, but their districts are peculiar; the head at New York is 114 feet, at Boston 123·5, at Hartford 121 feet, at Cleveland and at Louisville 150 feet, at Jersey City 158 feet, at Brooklyn

172 and 197 feet, at Albany 156 and 245 feet, at Watertown 245 feet; in these and other cases, the blame for defects in power of supply cannot be charged to want of controlling elevations, although some such instances are presented in the foregoing table.

With the care usually bestowed on reservoir location, it is strange that an equally important branch of the supply should either be overlooked in its details as a subordinate matter, or intentionally restricted, from mistaken ideas of economy, or from a desire to keep within certain general estimates of cost, by retrenching at the points usually last completed. For these, or similarly inexcusable reasons, it follows that distribution services, as a rule, are so imperfectly planned or laid, as to vitiate much of the benefit secured at the reservoir, and to grow worse and worse with increasing city consumption and increasing extensions of service mains.

The resultant evils of restricted head and delivery are due to inadequate *feeding mains*, in number, size and location, and to inadequate *service mains* in size, and also, in process of time, to the growth of tubercles wherever cast iron pipes are used.

Illustrations of this principle abound, and in most cases with aggravated circumstances. Water works which rival Roman Aqueducts in extent and cost, come far short of their true mission, when tried by lapse of time, and are eked out in service by hundreds of private engines at immense aggregate annual cost; works which boasted an hydraulic power able to supersede all fire engines and drive numerous factories and machine shops, are put to shame by the first "steamer" which connects with a fire-hydrant and has to put on its suction; the country abounds in extravagant aqueducts, engine houses, engines and reservoirs with appended systems of mains, which vitiate the greater part of the expended capital, and keep the element of health and safety almost out of reach; the city of New York with its investment of over twenty millions in supply, below Fourteenth Street and in the busiest region, is virtually exhausted, with a practical head at Canal Street, of not more than 60 feet above tide, as long ago as 1858; the City of Boston, with its superb Beacon hill reservoir in the very heart of its compactness, in twelve brief years, lapses towards the *cisternæ* of ancient times; good old, square blocked Philadelphia is ingenious in turning hydrants into suction-tubes; Hartford, spending $57,000 on her fancy engine house, cannot supply her charitable asylums; Louisville has $31,286 for a questionable stand-pipe, but no money for fire-hydrants; Detroit since 1857 has laid 77,498 feet of 2½ and 3 inch wood logs; Brooklyn, not yet relieved of her Construction Board, has already reduced her lines of eight inch mains to less than seven, and after a large outlay for protection vanishes, finds that four years at the furthest, is the limit of palliation from the tubercles; from such chapters of experiences, as matter of daily record these many years, one is taught what to avoid and what to improve.

Experimental illustrations of serious differences in effect in service mains or fire hose have abounded in practice, but do not abound in records of practice, as these are by no means voluminous. In one case

given in a Parliamentary Report, a 2½-inch stand-pipe attached London fashion, directly to the street main, gave, from a 9-inch main 7200 feet long, connected to 3000 feet of 15-inch, under a fixed head of 120 feet, through 40 feet of hose and a ⅞-inch nozzle, a jet 60 feet high; the same appurtenances, on a 7-inch main of 2400 feet, from 1500 feet of 9 inch and 600 feet of 12 inch, connected with 1650 feet of 15-inch main, under the same head, gave a 50 feet jet; while the jet from 600 feet of 4-inch main, with 600 feet of 5-inch, from the 9-inch main first mentioned, was 34 feet high. The aggregate length in the first case was 10,200 feet, and in the second 6150, and in the third 11,400, and between the first and third an additional length of 1200 feet, or not quite 12 per cent. reduced the jet from 60 to 34 feet, or over 43 per cent. the stand-pipe and hose friction being less in the third case than in the first, from diminished delivery. When it is remembered that a 9-inch pipe has not only about five times the calibre, but with the same hydraulic inclination, has nearly twice the velocity of a 5-inch, it is readily understood that calibre is an important item in maintenance of head on the attachment of street mains. As a farther illustration of the value of large feeding mains, we may cite an experiment on the 20-inch main at the Prospect Hill Engine House, in Brooklyn, in May, 1862.

Here the engine takes its supply from a 20 inch main of 1600 feet, leading from a 30-inch main of 3000 feet, and a 36-inch main of 26,062 feet, all being used directly for distribution except 15,637 feet of the latter which connects with Ridgewood Reservoir. With the engine pumping 3,750,000 gallons per day, and a rate of daily city draft* of not less than 9,250,000 gallons in addition, the dynamic head at 10 A. M. was 122 feet, (or about 42 feet frictional loss) taking the Reservoir at 164 feet. The maximum loss of head, for 5·8 miles of pipe, was about 25 per cent. at the time of greatest city draft.

This experiment, however, does not correctly indicate the dynamic head on the general service mains at the time, since the gauge was placed on the main at a point where it was itself feeding at the rate of 3,750,000 gallons per day, or more than one-quarter the entire reservoir delivery at the time. The gauge stood at 22 lbs, or 7·5 feet below the reservoir, before the engine started, and at 18 lbs or 16·5 feet below, at 11 P. M., with the engine at its regular work, but the city drawing only 10 per cent. of its demand at 9 A. M.

The office gauge in Halsey's Building, Jan. 24, 1865, at 10 A. M., stood at 21 lbs., with the pipe at rest, which is a ⅜ connexion with the street main of 8-inch calibre and about 80 feet in length. On opening its discharge it fell to 12½ lbs. with its current established; closing it, the index returned to 23 lbs. showing more correctly the street main head. As the gauge stands 83·5 feet above tide, the service main head was not less than 136·5 feet or 33·5 feet below the reservoir reported at 170 feet, or about 20 per cent. friction, the city

*This estimate of city draft at 10, A. M., is based on a statement in the Report for 1862, that "more than nine-tenths of the daily consumption is in the twelve hours from 5 A. M. to 5 P. M.," which is not confirmed by experience in other cities.

draft being estimated at 8,500,000 average per day, which would give a rate of 17,000,000 at the time. If we take the frequent gauge reading of 16 lbs. as the more correct index of this draft, the loss of head will be 45 feet or 26½ per cent.

One of the 6-inch jets of the Boston Common fountain in 1853, rose 90 feet above tide, its reservoir having a flow line of 120 feet; at this time however the city draft was slight, and the fountain is not very far from Beacon Hill.

From these and similar examples, two deductions may be made; first, that it is possible to maintain an extended distribution service from a distant reservoir, with a head within 30 per cent. of the starting point, and that as the demand increases, additional feeders will promply meet it; and second, that it is equally possible to vitiate more than 50 or 60 per cent. of the head by a defective service arrangement. The proper correctives are therefore clearly indicated.

The defects in this direction are to be traced to want of care in the arrangement of what are too often considered subordinate details; the most ample supply of water may have been secured, and there may be a superabundant reservoir head; it rarely happens with us, that these features are deficient; but if the distribution service is badly arranged as to feeding mains and calibre of service mains; if the hydrant branches are too small; if excessive friction occurs in hydrant tubes; if the hydrants are out of order; if long lines of hose are needed, because of improper intervals between hydrants, it is easy to comprehend why it happens so often in the history of burning buildings, that the losses of a single fire may far exceed all the expense attempted to be saved by such short-sighted and inadequate arrangements. What is true in the annals of conflagrations, in many of our prominent cities, as at New York in 1835, Albany in 1849, Troy in 1863, and other similar cases, (of neglect of proper supply) is true also, on a smaller scale, of neglect of proper arrangement; such parsimony being the very opposite to true economy.

Progress of Fire-Hydrants.—Having considered the objects and methods of distribution, the progress of fire engines, the conditions of fire service, and some of the defects of the present system, we come now to the progress made in hydrants and attachments.

From what has been said of the method of water delivery under the Low Service, as in general practice among the ancients, and the method of engine supply, it is easy to understand why the fire-hydrant, as we have it, is an attachment of recent date.

Matthews refers, in 1835, to the improved fire-service of London, and the use of fire-plugs, as follows:

"Whenever, or wherever, such calamities occur, a great abundance of water is almost instantly afforded, by means of the simple contrivance of pulling up the plugs connected with the principal mains, and thus the firemen are enabled to supply their engines, without intermission, either to prevent conflagrations from extending their ravages, or effectually to extinguish them."

The plugs thus described are illustrated by a plate, which represents a conical plug seat cast on the water main, into which a plug is

fitted, carrying a short handle, and surrounded with a wooden case, flush with the street pavement. No hose-nozzle is shown, and as the suction engines were in use in the previous century, the suction tube was probably dropped into the casing tube and was thus supplied, when the plug was drawn out; a process barely admissible with a low head, and inadmissible with high service. Since that time hydrants of various forms have been introduced, in some cases on the curb-line, as attachments to gas posts, but more generally as upright stand-pipes from the mains, with a turn-cock, and with a hose-nozzle, flush with the pavement. Under the Constant High Service of Hamburgh, the hydrant tube is attached to the side of the street main, so as to take a stop-cock, worked by a vertical rod from the street, the discharge pipe, 3 inches diameter in the clear, being curved up to street grade, where it ends in a nozzle-box, generally fitted with a two-way branch. At Glasgow, under a head of 225 to 450 feet, a hydrant is in use, as a stand-pipe terminating in a head with four nozzles, from which four lines of hose may be operated. This use of stand-pipes connected directly with the mains, and usually of 2½-inch calibre, is peculiar to European water works, under the Constant Service, and belongs to the class of contracted "flush" hydrants.

The operation of the common English fire-plug is thus described in 1850, in the Report of the General Board of Health:

> "With the ordinary plug it is necessary, on the discovery of a fire, to seek the turn cock, who is obliged, if the mains are full and a constant supply given, to seek the nearest main tap and turn off the water until the fire-plug is opened and the hose attached, and then go back and turn it on again. In the midst of the confusion much valuable time is thus added to that already lost in procuring the engines."

The original form of fire-hydrant under constant head in this country, may be traced to the year 1803, at Philadelphia, in a city which has the credit of being a pioneer in perfected water supply, and a pattern to all the rest. For this rank, Philadelphia is chiefly indebted to two men of genius, Benjamin Franklin, the philosopher, who advised, and Frederick Graff, the engineer, who planned and completed the noble work.

Although other and recent water works have taken a higher rank in scale of supply, this engineer must always be conceded the foremost place on the records of professional fame in hydraulic engineering, since his genius enabled him to put the Fairmount works far in advance of their European cotemporaries, in general design and perfection of detail, whether we examine the pumping machinery, the reservoirs, the arrangement of distribution service, or the details of water delivery. The studied arrangement of the pump-rooms with regard to future extension and lavish delivery; the subdivision of the reservoirs and their careful construction; and the abandonment of the niggardly policy of the European examples; the prompt substitution of cast iron mains; the general use of street washers, with hydrant pumps and other conveniences for city and individual comfort; and the use of hydrants, with hose nozzles, for engine supply or direct fire supply, under constant head, characterize, in each department, a

genius ahead of its age, of whose forethought those who follow after have not been slow to avail themselves. Listening to the cooling and lulling plash of the fountain in the beautiful grove on the banks of the Schuylkill, some quiet sunset hour, and studying the "wave theory" in the regular pulsations of its branching jets, one may well understand that an arduous professional life, as a labor of beneficence to others, may be full of its own rewards, and leaves a better and more enduring record than princely titles or gold-bought epitaphs.

The fire-hydrant referred to in the water laws of Philadelphia of 1806, is illustrated by an engraving published Oct. 1, 1817, "for the use of Hose and Engine Companies." This shows a cast iron hydrant, inserted by a tapering joint into a wooden main. The case was not shown, but was evidently used from the accounts paid for straw to pack it for the winter. The valve seat is placed at the base of the hydrant near the upper surface of the main. The valve drops in opening and is worked by a rod passing through the centre of the tube, with a stuffing box, saddle, screw-shaft, and handle mainly as in present use; the diameter of the tube is *larger* than that of the main by the diameter of the rod. The hose nozzle is fitted as a female screw. The frost vent is fitted near the valve-seat with a right-angle elbow, so that a plug-rod worked by hand may be raised from its mouth when the valve is shut, and frees the tube down to the main.

The Philadelphia fire-hydrant as illustrated Jan. 15, 1854, for the use of the Fire Department, is of similar type with that of 1806, except that it has a hydrant bend from the branch main, into which its base is leaded and held by a clamp collar and lugs, and that the frost rod is worked by the screw-shaft in place of by hand. It has these objections,—that the valve seat is thrown about 14 inches nearer the pavement grade, and that the frost vent, does not free the tube, as low as the valve-seat, or as low by 13 inches as the older form; with a branch calibre of 4 inches, the valve opening is also but $2\frac{1}{2}$. In both cases the hydrants have "stand-pipe" heads projecting about two feet above the pavement, the centre of the hose nozzle being about six inches above.

Recently an improvement has been made in the calibre of the Philadelphia hydrants, and in the addition of an air-chamber, to meet the requirements and modify the action of the steam fire-engines, as will be more particularly noticed under the special head of "calibre."

A form of main hydrant recently introduced at street corners in Brooklyn, with a tube of large calibre, has a puppet valve fitting a seat on the main, or a short casting introduced in the main line, which is worked by a centre-rod and terminates in a "flush" head carrying several hose-nozzles.

This general form of the Philadelphia hydrant of 1854, may be taken as an index of all others in common use, as to the use of a case, the internal valve motion, and the use of a frost vent, a hydrant bend between the branch and the tube, and a branch pipe from the main to the sidewalk. There are differences in detail, peculiar to different localities, of which a special analysis will be made in the following chapter on contingencies and defects.

(To be Continued.)

MECHANICS, PHYSICS, AND CHEMISTRY.

On the relative advantages of the Inch and the Metre as the Standard Unit of Decimal Measure. By Mr. JOHN FERNIE, of Leeds.

From the London Mechanics' Magazine, February, 1865.

The subject of a decimal system of measure resolves itself into two parts—the desirability of a decimal system, and the standard of measure to be adopted as the unit; the first of which may now be considered settled, and the principle definitely adopted in this country, the use of decimal measures being now legalized by a recent Act of Parliament. But the second part of the subject, the standard of measure, is still open, and is of very great importance; the consideration of it involves two preliminary scientific questions, and two practical conditions to be fulfilled.

In respect of the first scientific question—as to the standard that can be replaced best in case of being lost—there is no real choice between the metre and the inch: for the metre having been originally determined by measuring part of a quadrant of the earth's circumference, its length was also referred to the seconds pendulum for facility of repeating the measurement; and the inch being obtained from the seconds pendulum both the metre and the inch are thus verified by the same means; indeed the relation between them being once established, any means of verification is equally available for both. In regard to the second scientific question—as to the standard that is most universal in the character of its basis—the supposed advantage of the metre, as an even fraction of the quadrant, has been proved by the results of more accurate measurement to be a mistake, its actual length being an uneven fraction of the quadrant, just as the inch is an uneven fraction of the pendulum; and the length of the quadrant itself being different in different longitudes, there is therefore no choice between the metre and the inch, in respect of universality of its basis. The present legal standard of measure in this country is an individual metallic yard measure, independent of any reference to another source; and the metre is similarly a continuation or copy of an original standard metre which is now known to differ from the exact measure that it was intended to represent of the quadrant. There is no practical disadvantage, however, as regards accuracy in depending upon copying for the preservation of a standard; for by Mr. Whitworth's process of contact measurement, the accuracy in copying lengths can now be carried as far as one millionth of an inch, which is a higher approximation than can yet be attained in measuring the length of a pendulum or an arc of the earth's circumference.

The first practical condition to be fulfilled by the standard of measure is that it shall be the one best suited for use in decimal subdivision; and this point is to be determined by the relative practical convenience or inconvenience of its principal subdivisions and multiples. In connexion with mechanical engineering work, the inch has a special qualification for the standard of measure, since its subdivisions and

multiples predominate in the dimensions of the parts of machinery; it is the basis on which the various machines and engines made in this country have been constructed, and on which are founded calculations of strength of materials, sectional areas, steam pressure, power, velocity, capacity, and weight; so that the mechanical engineer may be said to think in inches, calculate in inches, and work in inches. For the classes of work in which the finer measurements are required, such as rifle bores, wire and metal gauges, &c., the desired degree of accuracy is readily and conveniently expressed in thousandths of an inch; whilst the millimetre, the smallest subdivision of the metre scale, not being smaller than 1-26th of an inch, requires the addition of two places of decimals to give the same degree of accuracy. This is a practical advantage of importance in favor of the inch as the unit of measure, since dimensions to one-thousandths of an inch are now required in regular use in mechanical work. Moreover, by taking as the unit the lowest of the present denominations—the inch—any longer dimensions on the present scale can be exactly expressed in the decimal system without fractional remainders. The second practical condition attaching to the standard of measure is that it shall be the one most extensively in use already, so as to involve the least alteration of existing measures; and in addition to a preponderance in the population now using the inch over that now using the metre, the former includes the great machinery producers, whose work already exists in such large quantities in all parts of the world in the form of engines, machinery, railway plant and tools; and the difficulties in the way of a change to the metre in this country appear, therefore, so insuperable as to amount practically to a prohibition of a decimal system, if it is to be based on the metre.

The general conclusions submitted in the paper in reference to the standard for decimal measure are, that, whilst the inch and the metre are equally eligible as regards their basis of reference, and could be replaced with equal accuracy in the event of being lost, the metre is not suitable for adoption in this country on account of its entire difference from the existing measures; and the inch is the most suitable standard of measure, from the extent to which it is involved in mechanical work, and from its convenience for expressing the smaller dimensions extensively used. For larger dimensions the most convenient decimal change would be the adoption of a 10-inch foot; and the larger measures being already multiples of the inch, their decimal adaptation to the inch would be at least easier than their entire alteration to the metre standard. It is also very desirable that the present weights and measures of capacity should be reduced to decimal systems; and it is considered that they can practically be based as readily upon the inch as the standard of measure as upon the metre, in the same way as with the definition of the metre or the inch.

In the discussion which followed the reading of this paper, the metre as the standard unit of decimal measure, in preference to the inch, was advocated by a deputation from the International Decimal Association, who concurred in considering that the question of the standard

of measure depended upon the fulfilment of the practical conditions which had been stated; that the standard should be the one best suited for use in decimal subdivision, and the one most extensively adopted already. As regarded decimal subdivision, the results of inquiries made by the Association had led them to recommend the metre as adapted for the greatest variety of measurements, and for the most numerous cases likely to occur in daily life; and to conclude that the inch did not in itself offer any advantage above the metre, even to mechanical engineers, since accuracy of measurement depended not on the scale, but on the measuring instrument employed, which ought to be applicable to any scale: and the millimetre had been already tried to some extent in this country, and was found convenient and suitable for mechanical work. In reference to the extent of population adopting the metre or the inch, it was believed that the numerical preponderance was already in favor of the former, and was steadily increasing by the more general adoption of the metre in other countries; and the simplicity and convenience of the metre system, both for measures and weights, were urged, together with the great importance of facilitating international communications, which were now so much interfered with by the incongruity of the systems in use.

The meeting then terminated. In the evening a number of the members and their friends dined together, in celebration of the 18th anniversary of the Institution.

On the reduction of Aluminium by Zinc.

From the London Mechanics' Magazine, January, 1865.

Aluminium would seem to be on the point of being very considerably cheapened. The largest item of its cost hitherto has been that of the sodium used for its reduction, but we are now told that M. Basset, of Paris, has succeeded in reducing it from its chloride by means of the very much cheaper metal, zinc. The idea of using zinc to reduce aluminium is not at all new—it was patented in this country, as a communication from abroad, as far back as 1854, but no one before M. Basset ever succeeded in practically realizing it. M. Basset's plan is to fuse chloride of aluminium with an *excess* of zinc, and he states that the results are chloride of zinc and an alloy of zinc and aluminium from which all the zinc may be driven off by a white heat. If this process be really practicable on the great scale, there will be no reason why aluminium should not speedily become cheap enough for employment in the many mechanical applications for which it is so admirably fitted, instead of being confined, as at present, to ornamental uses only.

There are other metals than aluminium in respect of which zinc seems likely to prove useful as a reducing agent. Last spring M. Poumarede forwarded to the Academy of Sciences of Paris, a communication respecting the use of its vapor as a means of reducing the more difficultly reducible of the heavy metals, as chromium, cobalt, nickle, and manganese. He stated that it reduces all these metals,

especially from their chlorides or fluorides, quite easily, and we have recently seen his experiments repeated in London with the fullest success.

Description of a Cheap Form of Automatic Regulator of the Electric Light. By Mr. Samuel Highly.

From the London Athenæum, Oct., 1864.

The principle of this "Pneumatic Electric Regulator" was suggested to the author by Mr. Malden. The instrument is sensitive in action, and, from its simplicity, little liable to get out of order, and can be arranged for any length of carbon. The rod supporting the upper carbon is attached to a copper float, which rests upon a column of water, contained in a chamber communicating by an opening with an air chamber, from which a pipe, terminated by a flexible tube of vulcanized rubber, is carried under a wedged-shaped piece attached to the rod holding the lower carbon, and which passes through a stout coil of insulated wire. When the carbons are brought into contact the current passes through, and the coil becomes magnetic, pulls down the iron core, and separates the carbon, so as to produce the proper arc of light; at the same time forcing down the wedge upon the flexible tube, closing it as effectually as with a stop-cock. As soon as the distance between the poles becomes too great for the current to pass freely, the coil ceases to be magnetic, and the lower rod is raised slightly by means of a lever and counterpoise spring. Air is thus forced from the chamber by the column of water; the float sinks, bringing down the upper carbon into contact with the lower one; the current is thus again completed; the coil becomes magnetic, and pulls down the iron core, pressing the stop-cock wedge upon the upper tube. These operations are repeated sympathetically as the carbon burns away.

The Magnesian Light.

From the Lond. Civ. Eng. and Arch. Journal, Jan., 1865.

A lamp for the purpose of burning the wire is said to have been invented by Mr. A. Grant, who is endeavoring to bring his design to practical perfection. Mr. Grant seeks to make magnesium cheaper still than the best stearine, and states that by burning a strip of zinc in conjunction with two strips of magnesium he is able to reduce the cost of the light by two-thirds. He even ventures to predict that magnesium will become as cheap as zinc, and that in the course of time it will be possible to illuminate a street a mile long, at the rate of half-penny per hour! It is not a small thing to be able to record that photography is no longer dependent upon the action of the sun. The value of magnesium as an illuminator for the purpose of "signaling," is too obvious to escape immediate recognition. The portable nature of the contrivance, and its perfect immunity from risk of explosion, together with some other evident advantages, render its vivid

light all the more practically valuable. The Parisians, we are told, are going to make it figure on the stage, and have sent orders for some of Mr. Grant's lamps to illuminate their painted scenery.

A New Blasting Gunpowder. By M. H. SCHWARTZ.

From the London Chemical News, 267.

This powder is now largely employed in mining. Its combustion is slow, but complete. The following analyses show why it is cheaper than ordinary powder:

	I.	II.
Soluble salts, . . .	74·55	74·32
Nitrate of potash, . . .	56·22	56·23
Nitrate of soda, . . .	18·30	18·09

The treatment by sulphide of carbon produced:—

Dissolved sulphur, . .	9·68	7·61
Carbon remaining, . .	14·14	15·01
Moisture, . . .	1·78	11

It is a coarse-grained powder, in which one part of potash nitre is replaced by nitrate of soda.

In the first instance, one part of nitrate of soda for one part of nitrate of potash was used, but it was afterwards found best to employ a third of nitrate of soda.—*Bulletin de la Société Chimique.*

On Joule's Method of Testing Steam Boilers by Hydraulic Pressure.

From Newton's London Journal of Arts, March, 1865.

The President drew attention to the late fatal explosion at Peterborough, and asked whether the easy method of testing steam boilers, described some years ago by Dr. Joule, was forgotten, or found to be impracticable?

Dr. Joule said, that he had taken pains to give his method—by which the testing by hydraulic pressure could be applied with the utmost facility, by simply filling the boiler with water, and then raising its temperature a few degrees—a very extended publication. He believed that the objection raised by some to its use, was the absurd one that hydraulic pressure injured the boiler. The very object of a test was to detect weak boilers, for the purpose of strengthening or rejecting them. He was at a loss for terms strong enough to express his opinion of the reckless disregard of life, or the ignorance which resulted in the deplorable catastrophes which were constantly occurring; and he believed that the only method of cure would be that proposed by Mr. Binney, in the case of the explosion of fire-damp in mines, namely, that the parties to blame should be compelled to support the widows and orphans of their victims.

Mr. Alderman Pochin stated that he had made use of Dr. Joule's plan, and found it quite practicable and easy of application.

Proc. Manchester Liter. and Philos. Soc. Feb. 7, 1865.

For the Journal of the Franklin Institute.

On Work and Vis-viva. By J. W. NYSTROM, Act. Chf. Eng., U. S. N.

In a previous article on Physical Work, a continuance of the same subject was promised, extending to the combined action of gravity and mechanical force, the manuscript of which has since been lying on the shelf, awaiting some criticisms on the part already published.

In the great confusion which now involves the subject, it was deemed necessary to advance cautiously, having each step of progress thoroughly discussed as far as it went; for experience admonishes us, that any reform, however simple or important, cannot be introduced and accepted without due deliberation.

The liberal criticisms of Prof. De Volson Wood, however much at variance in doctrine with my own, were perused with unaffected satisfaction, and should the interest in the subject evinced by him be followed by others, we might hope that valuable light would ultimately be shed upon it.

The greater part of his criticism is derived from misconception of the meaning of the letter V. In work ($W = F\,V\,T$), V means the velocity of the force F; or if F and V either, or both, are variable, then they can only signify the *mean* force, and *mean* velocity in the time T. In the member $M\,V^2$, V means the uniform velocity of the moving body M, after the force F has ceased to act upon it; or the final velocity imparted to the body M by the force F at the time T.

Prof. Wood says, "*the member* F V T *does not express the work which the force* F *does upon a body free to move, in producing the velocity* V *in time* T; *but when the body is free to move, as here supposed, it equals one-half the work.*" In this he is in error. F V T equals twice the work when V means the final velocity of the constant force F at the time T, because the *final* velocity of the force F is just twice its *mean* velocity in the time T, as will be proved hereafter.

Prof. W. says. "*If still further we admit that* $F\,T = S$ *and* $V^2 = 2\,g\,S$, *we have by substitution and reduction,* $F = 2\,W$ *which cannot be true.*" This is most probably a misprint, and Prof. Wood means $F\,V = S$, in which case V must be the *mean* or uniform velocity in the space S; but in the formula $V^2 = 2\,g\,S$, V means the *final* velocity in the space S, which reduces $F = W$, which *is* true. Prof. Wood by W means weight. He says "*We see then that the equation* $F\,V\,T = M\,V^2$ *reduces to an absurdity under every hypothesis except the first, and in that, it simply gives a true equation without expressing the value of work.*" Is it possible that the formula can be a true equation of work without expressing the value of work? It gives precisely the same value of work as *vis-viva*; still he says; "*We see then that it never expresses the true relation between work and vis-viva.*" I contend that it does, and shall proceed to prove it in this article.

If I am not very much mistaken, Prof. Wood is confused in his conception of *force*, *power*, and *work*. He says that "*work was called by Smeaton, Mechanical Power, and by Monge and others Dynamic*

Effect." I understand those authors to mean F V for mechanical power and dynamic effect, and not F S, which is work, and regret that I have not succeeded in making Prof. W. understand the difference between power and work, and that F V T is work. He admits that S = V T, when V is constant; he also admits that work is F S; will he then not allow us to insert V T for S in work, which will make F V T? Professor Wood further says "*Now the work alone in the space* S, *is the same, whether it be done in one hour, day, or week; it is independent of the time.*" Then he confounds horse-power with work, in which he says time *is* included! This radical error has been persistently maintained in antagonism to me by Professors and Engineers, from a date long antecedent to my article on Work as published in this *Journal*; and Professor Wood will have no difficulty in finding supporters in that doctrine, for as accepted by him it is taught in schools and colleges. I have in the last eight months had frequent occasion to discuss the subject with Engineers, some of whom were apparently fresh from college and have studied *Weisebach's, Bartlett's and Moseley's Mechanics*; and who not only differ with me on the subject, but also disagree amongst themselves. They all seem to agree that *time is not* included in work, and the few who acknowledge the existence of power as a different quantity from work, also agree that time is included in power, but not in work. They argue that "the unit of power is 33,000 pounds lifted one foot per minute, whilst work is one pound lifted one foot independently of the time." They also assert that space is a simple element, and that velocity is a compound result of time and space.

My argument on the contrary is this; that the space one foot is the product of *time* and *velocity*, and when we say "*per time*, in the unit of *power*, we divide the space by the time, and the remainder is only pounds and velocity. Therefore in the ordinary conception of work F S, the *time* and *velocity* are included in the space S.

Let F = weight or force in pounds; v = velocity in feet per second of the force F, t = time in seconds; F and V being constant or the mean in the time t, S = space in feet traversed by the force F; P = power expressed in foot-pounds of power; W = work in foot-pounds of work.

The popular expression of power is

$$P = \frac{FS}{T}, \text{ but when } S = vt,$$

$$\text{we have } P = \frac{Fvt}{t} = Fv, \text{ the primitive formula for power.}$$

The popular expression of work is,

$$W = FS, \text{ but when } S = vt,$$

$$\text{we have } W = Fvt, \text{ the primitive formula for work.}$$

These are the fundamental formulas throughout the science of dynamics. They include the whole conglomeration of *Vis-viva*, the different momentum and quantities of motion, activity, inertia, energy, dynamic effects, &c., &c., as shall hereafter be proved.

It is perfectly absurd to say with Prof. Wood and others, "*that work is independent of time.*" If that were so, why then is *time* such an important item in contracts for work? and why do we so often fail to accomplish a contracted work in a specified time? In digging a canal, the power in operation, $P = Fv$, is represented by the number of laborers, and the quantity of work accomplished will be direct as the time. Prof. Wood may do a work in one minute, in one day, or in any time he pleases, the work accomplished will, in all cases, be Fvt, and nothing else. For a given quantity of work, *time* can vary, only at the expense of force or velocity. On Prof. Wood's reasoning we can say that force is independent of work, because a force of one pound can accomplish as much work as a force of 100 pounds; but at the expense of time and velocity only. Cannot the Professor understand and admit, that space cannot be accomplished without time and velocity? He may easily satisfy himself in simple experiments by drawing chalk-lines on the black-board.

Prof. W. maintains that "*the English unit of work is* 33,000 *pounds raised one foot per minute.*" This is the English unit for power, and not as he erroneously assumes, for work.

Power is the rate at which work is executed.

Power is working or labor, and the result of labor is work.

Power is the differential of work.

Work is the integral of power.

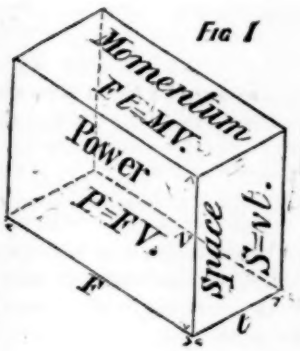


Fig 1

The subject may be better illustrated as follows:

The three dimensions, *length*, *breadth* and *thickness* in this parallelopipedon, may be assumed to represent the elements *force* F, *velocity* v, *and time* t. The area of the side elevation Fv, therefore represents the power. The area of the end elevation vt represents the space; the area of the base Ft the momentum; and the cubic contents $Fvt = FS = Pt$, represents the work, provided that the force F and velocity v, are constant in the time t. But when F and v are either, or both, variable, they will generate and be represented by various other figures. In the case of a constant force F, acting on a mass free to move, the velocity will be variable with a uniform acceleratrix, then the work done will be represented by a wedge.

Let a constant force F, fig. 2, act on a mass or body M, in the direction of the arrow, F and M being expressed by the same unit of weight, say pounds, and no other force acting on M; then we know from the law of gravity that the acceleratrix of the force F will be

Fig 2

$$G = \frac{gF}{M}.$$

Let the force F act on M for any length of time T, then we know from the law of gravity that the final velocity attained will be,

$$V = GT.$$

Let v denote the velocity at any time t, both less than V and T, then the power in operation will be,

$$P = Fv,$$

and the differential work will be,

$$dW = Fv\,dt,$$

but $v = Gt$ and

$$dW = FGt\,dt,$$

when

$$W = \int_{t=0}^{t=T} FGt\,dt.$$

$$W = \underset{1}{\frac{GFT^2}{2}} = \underset{2}{\frac{FVT}{2}} = \underset{3}{\frac{FV^2}{2G}},$$

which are the three forms of work accomplished by the constant force F, acting on the body M in the time T.

We know that $F = \frac{GM}{g}$, which inserted in the above members, will give,

$$W = \underset{4}{\frac{G^2MT^2}{2g}} = \underset{5}{\frac{GMVT}{2g}} = \underset{6}{\frac{MV^2}{2g}}.$$

These are the three forms of work concentrated in the moving body M.

Comparing the 2d and 6th members we have,

$$\underset{2}{\frac{FVT}{2}} = \underset{6}{\frac{MV^2}{2g}};\ \text{or}\ \underset{7}{FVT} = \underset{8}{\frac{MV^2}{g}} = 2W,\ \text{which was to be proved.}$$

When M denotes the mass of the body, we have $FVT = MV^2$

This is the equation which Prof. Wood in his criticism proved to be "*an absurdity under every hypothesis.*" I cannot omit to state that his reasoning must be very elastic.

The member MV^2 is the mysterious quantity *Vis-viva*, which I shall endeavor to bring bodily to sight.

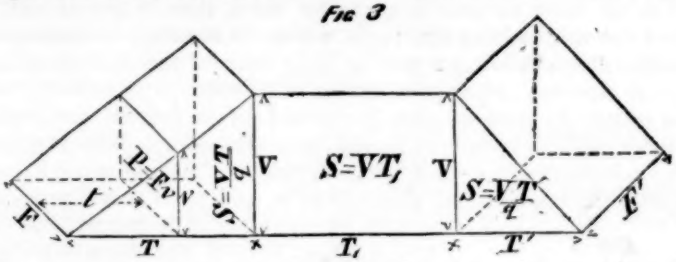
The velocity V in the preceding formulas, does not signify the *mean* velocity of the force F, but the *final* velocity imparted by the force F to the body M, in the time T; or the uniform velocity of the body M after the force F has ceased to act upon it. This *final* velocity at the time T, is just twice the *mean* velocity of F in the time T, when F and the acceleration G are constant as here supposed.

An illustration may form a clearer conception of the nature of the preceding formulas.

Let the line F, fig. 3, represent the magnitude of a constant force acting on a body free to move in the direction t; the line T to represent time; any ordinate v to represent the velocity of the force F at any corresponding time t, until F ceases to act, when the *final* velocity V will

be constant. The hypothenuse for the catheters T and V, will become a straight line when the acceleration of F is constant. Then the area

Fig 3



of the cross section $P = Fv$ represents the power in operation at the time t; the area of the triangular side represents the space $s = \frac{vt}{2}$, passed through by the force F; the work done in the time t, is represented by the cubic contents of the wedge $\frac{Fvt}{2}$, or for the whole time T,

$$W = \frac{FVT}{2}.$$

When the force F ceases to act at the end of the time T, the body will continue with a uniform velocity V, and pass through the space $S = VT_1$, with its concentrated work $W = \frac{FVT}{2}$, until we will suppose some force F′ is applied to stop it.

The linear space S which the body passes through in the time T_1, is on the figure represented by the rectangular area VT_1, constituting the elements of space.

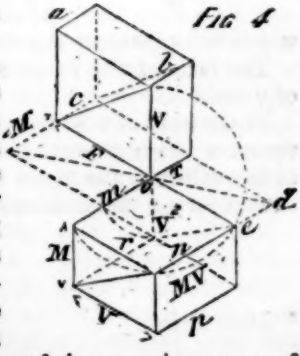
The body will come to rest when the work of the opposing force F′ is equal to the work accomplished by the force F, or when $\frac{1}{2}F'V'T' = \frac{1}{2}FVT$. Let the common factor $\frac{1}{2}V$ be eliminated, and the momentums $F'T' = FT$, when the body comes to rest.

It has before been proved that,

$$2W = FVT = MV^2,$$

and the momentum $FT = MV$.

Fig 4

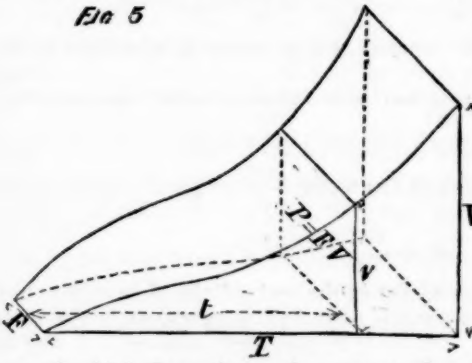


Now let the parallelopipedon *a*, *b*, T, *c*, fig. 4, represent FVT, which as before shown, is double the work, when V = *final* velocity; then the base will represent the momentum FT. Continue the line F from *o* to *e*, make $oe = V$; complete the rectangle T, *d*, *e*, draw the diagonal *d*, *o*, continued to M; *c* and M being in a straight line; then M represents the magnitude of a mass in which is accumulated the work $\frac{1}{2}FVT$. Complete the square V^2, and draw on it the figures *m*, *e*, *p*, V, M; which then represent the so-called *Vis-viva* MV^2, in a moving mass, of which only one-half, or the wedge *m*, *n*, *r*, is realized as substance, while the other half, or the wedge *n*, *r*, *p*, remains in mystery.

Vis-viva is the most unfortunate term in dynamics, inasmuch as it does not express what it means, neither in substance nor in quantity. There is no more *vis-viva* in a moving mass, than in one at rest. It requires F*vt* and nothing else to set a mass in motion; it requires F*vt* and nothing else to bring a moving body to rest; and it is F*vt* and nothing else that can change the motion of a body.

The sooner the term *vis-viva* is rejected in its present acceptation, the sooner will the science of dynamics be cleared up. Whatever name may be selected for the work concentrated in a moving mass, it ought to express the fundamental elements F, V, and T. I would propose to call it energy, but even if the term *vis-viva* is maintained, it ought to mean the true work $\frac{1}{2}MV^2$, and not MV^2.

Fig 5



A parallelopipedon constructed on the mean velocity will represent the true work F*vt*.

When the force F is variable, as represented by fig. 5, the hypothenuse of the catheters F and V, will be a curved line, the form of which depends on the nature of variation of F, but the work will still be

$$W = \int F G t\, dt, \text{ as before.}$$

Momentum, $MV = \int F\, dt.$ Velocity, $V = \frac{1}{M}\int F\, dt.$

Space, $S = \int dv \int F\, dt.$ Power, $P = \frac{F}{M}\int F\, dt.$

Work, $W = \int P\, dt = \Sigma F v t = \frac{MV^2}{2g},$

which will represent the work under all circumstances.

The form of the figure of work depends on the nature of variation of F and *v*.

In the case of a body moving through a fluid, as a vessel through the water, a bomb through the air, where the resistance is as the square of the velocity, the figure of work will be a pyramid.

Comparing the members 1 and 5, page 184, we have,

$$\frac{GFT^2}{2} = \frac{GMVT}{2g}, \text{ or the}$$

Momentum,

$$FT = \frac{MV}{g},$$

For circular motion we have the velocity,

$$V = \frac{2\pi Rn}{60} \qquad 11$$

which inserted in the member 10, will be

$$FT = \frac{M2\pi Rn}{60g}, \qquad 12$$

in which R = radius of gyration of the rotating body, and n = number of revolutions per minute.

A force multiplied by the lever it acts upon, is called *static momentum*; which is analogous to the force of inertia multiplied by the radius of gyration MR, which seem to have a claim to be called *moment* of *inertia*.

The square of the member 11, inserted in the member 6, will represent the work concentrated in a revolving mass, or

$$W = \frac{M4\pi^2R^2n^2}{3600\times 2g} = \frac{MR^2n^2}{5866{\cdot}8},$$

of which the part MR^2 has been denominated moment of inertia.

I was in error in my statement that "Moseley called moment of inertia, Work." I ought to have said that the substance of the so-called moment of inertia MR^2, is work when n is constant.

Prof. Wood refers to his two articles on *Vis-viva*, published in the November number, 1862, and in the February number, 1864, of this Journal. I beg leave to state that the confusion existing in those articles was one of the many reasons why I undertook to write on the subject, which was stated in the October meeting of the Franklin Institute; for in both those articles, as well as in the general criticism on mine, *force*, *power* and *work*, are confounded with each other.

The article in the November number, 1862, page 351, commences in this way: "*When we consider that eminent scholars have taken different views upon the measure of the force in a moving body, it is not strange that students, while yet in the elements of mechanical science, should be at a loss to know whether the force varies as the velocity or as the square of the velocity.*" Here the Professor confounds force with work and momentum. The force of inertia in a moving mass is equal to whatever external force is applied to change its motion. The different views taken by eminent scholars and students are, whether *power* or *work*, is as the velocity or as the square of the velocity. The Professor continues, "*They do not see why the momentum does not express the force as well as the Vis-viva; or they fail to distinguish between the two forces.*" Force is neither momentum nor *Vis-viva*. Force is, as far as we yet know, a simple element; whilst momentum is a compound of two elements, and *vis-viva* a compound of three elements. The momentum in question is derived from the analogy

$$F : M = V : T, \text{ or } FT = MV.$$

The momentum FT is equal to the momentum MV. It is the momentum MV which the Professor calls one kind of force as different from MV^2, another kind of force! The momentum MV is substantially the same as the product obtained by multiplying six horses by four bushels of barley; it must be multiplied or divided by some element before it can he recognised as a physical quantity; if we multiply MV by V, the product will be work; multiply MV by $\frac{G}{g}$, the product will be power; divide it by T and it will be force; divide it by F and it becomes time. If 6 horses can eat 3 bushels of barley, how many horses can eat 4 bushels? Here we multiply 6 horses by 4 bushels of barley, which will be only a momentum to be divided by 3 bushels of barley, when the result will be eight horses. On the same page, 351, Prof. Wood says, "*The force which is stored in a moving body, is a force of inertia; hence it is equivalent to the force which would cause the body to move from a state of rest to a velocity* V, *when free to move without any resistance.*"

It is most difficult in this case to distinguish if the Professor means force or work. The force of inertia in a body free to move, is equal to any external force applied to change its motion; but the force which has set the body in motion has no necessary relation with the force which may bring it to rest, except with consideration of time, as in case of gravity acting on a pendulum; but the work expended in setting a body in motion, is equal to the work required to bring it to rest.

On the next page, 352, Prof. Wood gives an example of momentum by multiplying the weight of a moving mass 200 lbs by its velocity 2 feet, and calls the product 400 lbs feet, but does not state what kind of foot-pounds. Can this momentum lift 400 lbs one foot high in a unit of time or independently of time? Next the Professor gives an example of *Vis-viva*, by multiplying the same mass 200 lbs by the square of its velocity or $200 \times 2^2 = 800$, but does not state what kind of substance this 800 is. From the commencement of his article we are led to suppose it to be force. The fact is that the *Vis-viva* 800 is in substance foot-pounds of work; while the momentum 400 multiplied by $\frac{G}{g}$ will be in substance foot-pounds of power. The Professor, however, admits that his example may appear paradoxical. The difference between foot-pounds of power and foot-pounds of work, is the same as the difference between square feet and cubic feet. Prof. Wood thinks that "*the reason why much obscurity has hung over the subject, is principally on account of brevity of treatment by most authors.*" I do not believe that such is the case, but is owing rather to a want of clear conception on the subject, which all our treatises testify by their unnecessary complication and confusion. Professor W. gives me credit for the definition of *power*, to be "*force* multiplied by *velocity*." I learned that at the Royal Technological Institute in Stockholm, when I was a boy, and it is so used by all practical steam engineers throughout the world, for calculating the power of steam engines. The force on the steam piston multiplied by its velocity, is the power of the engine.

The definitions of *force*, *power* and *work*, are intended only to hold good in the science of dynamics. I will not undertake to define those terms as they are used in popular language. The only new term I proposed in my article, was *Workmandays*.

In the first formula for work under action of gravity, page 331 last volume, the number 2 is left out in the denominator. It should read,

$$W = \frac{M V^2}{2g\,550 \times 3600}$$

On the Chemical History and Application of Gun-Cotton. By Prof. ABEL, F.R.S., Chemist to the War Department.

From the London Chemical News, No. 236.

Continued from page 138.

A rapid current of air will also effect the transformation of the combustion of gun cotton from the ordinary to the slow form if the yarn be enclosed in a moderately wide glass tube, with one end protruding from the tube so that it may be inflamed in the ordinary manner, but, unless the current be very rapid, an explosive mixture of air and the inflammable gases generated from the gun cotton may be produced in the tube, and become ignited, in which case the gun cotton will flash into flame instantaneously, and the tube will be shattered by the explosion. If, however, a long thin piece of gun cotton-yarn be passed through a small, narrow, glass tube, one or two inches long into which it fits so loosely that it may be drawn through very easily, the change in the form of combustion is effected with certainty and without the aid of a current of air. When the gun cotton thus arranged and placed upon a flat surface is inflamed at one extremity, it burns as usual until it reaches the one opening of the tube; the slow form of combustion then takes place within the tube, and the gun cotton will continue to burn in the slow manner, emitting only the small tongue of flame after the combustion has reached the portion of yarn on the other side of the tube, which will be entirely burned in this peculiar manner. In fact, to change the ordinary into the slow form of combustion of the gun cotton-yarn *in open air*, it is only necessary to pass a piece of the material through a perforation in a diaphragm of wood, card-board, or paper, and to allow it to rest upon a flat surface on both sides of the diaphragm. The gun cotton will burn as usual on one side of the screen, until its combustion reaches the perforation, when the large bright flame will vanish and the gun cotton upon the other side of the screen will burn in the slow manner to the end.

The two last experiments show that, if the combustible mixture of gases evolved by the action of heat upon gun cotton when it is inflamed in open air are prevented, even for the briefest space of time, from completely enveloping the burning extremity of the yarn or twist; or, in other words, if they are forced for an instant to escape only in a direct line with the burning surface of gun cotton from which they are

emitted, those particles of the latter which are in immediate proximity to the burning portion cannot be raised to the temperature necessary for their rapid and more complete combustion, and hence the gases themselves are, in turn, not supplied with sufficient heat for their ignition. Now, as the gases which escape unburned convey away a very large portion of the heat developed by the metamorphosis of the gun cotton, it is impossible for the latter to continue to burn otherwise than in the slow and imperfect manner. If, however, a flame or highly heated body be held in the path of the gases as they escape, they will at once be ignited, and the yarn will burst into the ordinary form of combustion. The correctness of this explanation may readily be demonstrated by two or three simple experiments. Thus, if a piece of loose or open gun cotton-yarn is employed in place of the compact material which furnishes the results just described, it is very difficult, or even impossible, to cause the rapid combustion to pass over into the slow form, because the escaping gases cannot be diverted all into one direction, and cannot, therefore, be prevented from transmitting the heat necessary for perfect combustion from particle to particle of the material. Again, if a piece of the compactly twisted gun cotton-yarn, placed upon a flat surface, is inflamed in the usual manner, and a jet of air is then directed in a line with the gun cotton so as to meet the flame, the latter will appear to be blown out, though the cotton still burns; in fact, the burning gases are prevented for an instant from completely enveloping the extremity of the gun cotton, and hence the combustion at once passes from the quick to the slow form. Conversely, if, when the yarn has been made to burn in this slow manner, a very gentle current of air be directed against the burning portion so as to force back upon the latter the gases which are escaping, thus impeding the raqid abstraction of heat, it will very speedily burst into the ordinary form of combustion, because, under these circumstances, the gases are almost immediately raised to the temperature necessary for their combustion. In the same way, if a piece of the yarn, placed upon a board, be made to burn in the slow manner, and one end of the board be gradually raised so that the burning extremity of the gun cotton is the lowest, the latter will burst into flame as soon as the board has been raised to a position nearly vertical, so that the escaping gases flow back upon the burning surface.

The slow or imperfect form of combustion may be at once induced in the compact gun cotton-yarn, in open air, by applying to any part of the gun cotton a source of heat not sufficiently great to inflame the gases generated. A wire or metal rod, heated to any temperature between 135° C. to just below visible redness, or the spark of a thin piece of smouldering string, will invariably produce the result described. Of course, this effect, like most of the phenomena described, is to a considerable extent dependent upon the mechanical condition of the gun cotton, upon the relation between the *quantity* as well as the *degree* of heat applied, and the amount of surface of the gun cotton, and upon other conditions. While a small spark, or a thin platinum wire heated to full redness, only induces slow combustion in the com-

pact gun cotton-yarn, a thick rod of iron heated only to dull redness will invariably inflame it in the ordinary manner. A piece of open yarn cannot be ignited so as to burn in the slow manner; on the other hand, the more compactly the gun cotton is twisted, the more superficial is the slow form of combustion induced in it; indeed, the gun cotton may be rendered so compact that it will simply smoulder in open air, if ignited as described, leaving a considerable carbonaceous residue; and the heat resulting from this most imperfect combustion will sometimes be abstracted, by the escaping gases, more rapidly than it is developed, so that the gun cotton will then actually cease to burn, even in open air, after a short time.

The remarkable facility with which the effect of heat upon gun cotton may be modified, so as even to produce results totally opposite in their characters, as exemplified by some of the experiments which have been described, renders it easily conceivable that this material may be made to produce the most varied mechanical effects, when applied to practical purposes; that it may, indeed, be so applied as, on the one hand, to develope a force, very gradual in its action, which may be directed and controlled at least as readily as that obtained by the explosion of gunpowder; while, on the other hand, it may be made to exert a violence of action and a destructive effect far surpassing those of which gunpowder is susceptible. The results arrived at in Austria, which show that gun cotton can be made to produce effects from three to eight times greater than those of gunpowder, cease to be surprising after a study of the chemical and physical characteristics of this interesting explosive agent.

The products obtained by the explosion of gun cotton and its decomposition under various conditions have as yet been very imperfectly studied, but there is little doubt that they vary in their nature almost as greatly as the phenomena which attend the exposure of the material to heat under different circumstances. It is well known that when gun cotton is inflamed in the open air there is produced—in addition to water, carbonic oxide, carbonic acid, and nitrogen—a considerable proportion of binoxide of nitrogen, so that the gaseous mixture assumes a red brown tinge, and becomes very acid, when it mixes with air. The products of the varieties of imperfect combustion which gun cotton has been described as susceptible of undergoing are undoubtedly much more complex in their character than those just referred to. They include at times a proportion of some substances not yet examined, which make their appearance as a white vapor or smoke. Cyanogen can readily be detected in all the products of imperfect combustion. The proportion of binoxide of nitrogen is generally so large that the gaseous product becomes very highly colored when mixed with air. Peroxide of nitrogen has also been observed in some instances. Lastly, there is little doubt that the products occasionally include a proportion of oxidizing gases.

The products which have just been alluded to are the results of the decomposition of gun cotton either at ordinary or diminished atmospheric pressures. When the explosion of the material is effected in

a confined space, in such a manner that the main decomposition takes place under pressure, the metamorphosis which the material undergoes is of more simple and complete character.

It has been found by Karolyi that when gun cotton is exploded by voltaic agency, in a shell which is burst by the explosion, and which is enclosed within an exhausted chamber so that the products of decomposition are collected without danger, the results obtained, under these conditions, are comparatively simple; the analysis of the contents of the chamber after the explosion showed that they consisted of—Carbonic acid, 20·82 per cent.; carbonic oxide, 28·95; nitrogen, 12·67; hydrogen, 3·16; marsh gas, 7·24; water, 25·34; and carbon, 1·82. The decomposition of gun cotton under these conditions, which are similar to those of its explosion when employed as a destructive agent, appears therefore, not to be attended by the production of any oxide of nitrogen. The lecturer found in some preliminary experiments made under the same conditions as those of Karolyi, that only a minute proportion of binoxide of nitrogen was produced. These results, when compared with those obtained by the ignition of gun cotton in open air and rarefied atmospheres, show that, just as the decomposition of this material is of a more complicated and intermediate character, in proportion as its combustion is rendered imperfect by diminution of pressure or other circumstances, so, conversely, the change which it undergoes will be the more simple, and its conversion into gaseous products the more complete, the greater the pressure, beyond normal limits, under which it is exploded—that is to say, the greater the resistance offered to the generated gases upon the first ignition of a charge of gun cotton (and, consequently, the higher the temperature at which the decomposition of the confined gun cotton is effected). It is, therefore, readily intelligible that the notions hitherto generally entertained with regard to the very noxious character of the products of explosion of gunpowder and their powerfully corrosive action upon metals, based as these notions have been upon the effects observed on exploding gun cotton in open air, have been proved to be erroneous by the results of the actual application of gun cotton to artillery and other purposes. The foregoing considerations contribute, moreover, to the ready explanation of the fact established by the experiments in Austria, that the destructive effect of gun cotton is greatly increased, within certain limits, by increasing the resistance which the products of explosion have to overcome before they can escape into the air.

The conditions (of temperature, pressure, &c.) which influence the nature of the decomposition of gun cotton, exert, unquestionably, a similar influence upon the nature of the explosion of gunpowder, and upon the mechanical effects which its products are capable of exerting. Observations made by the lecturer in experiments upon the ignition of gunpowder in rarefied atmospheres point to the existence of products of comparatively complicated character among those found by the gradual decomposition of that material, under the conditions described. The earlier investigators (Guy Lussac, Chevreul, &c.) of the products of explosion of gunpowder represent these as being of very simple

character, and in harmony with the theory that gunpowder is converted essentially by its explosion into carbonic acid (or a mixture of that gas and carbonic oxide), nitrogen, and sulphide of potassium. But more recent experimenters, Bunsen and Schischkoff, who have made a very elaborate examination of the products which they obtained by the explosion of gunpowder, represent the change to be one of a very complicated character; fix the percentage of solid substances found at a much higher figure than that hitherto accepted; and show that the sulphide of potassium, which has hitherto been considered as the principal of these products, was only produced in very small proportion in their experiments. The conditions under which these chemists exploded the gunpowder did not, however, correspond in their character at all to those under which gunpowder is exploded in actual practice, and would, therefore, be very likely to furnish results greatly at variance with those produced when a charge of powder is fired in a gun, a shell, or a mine. That sulphide of potassium is abundantly produced upon the discharge of a firearm appears beyond doubt; it may be readily detected in the solid matter which remains in the barrel near the breech; it may be found deposited in considerable quantity near the muzzle of the arm; and there appears strong reason for believing that the flash of flame observed at the mouth of a firearm, upon its discharge, is due in part to the ignition, as it comes into contact with the air, of sulphide of potassium, which has been vaporized by the heat of the explosion, and is thus mixed with the escaping gases.

In comparing the effects of gun cotton, as an explosive agent, with those of gunpowder, and in basing theories with regard to the difference in the mechanical effects exerted by the two, upon the analytical results of the products of their explosion which have been obtained up to the present time, it is necessary to proceed with great caution; for exceptional results cannot form any sound basis for correct theories or tenable arguments. It can only lead to incorrect conclusions, which may considerably retard the thorough investigation of a most important subject, if the facts be ignored or lost sight of—that, first, the conditions which practically influence the nature of the products of the explosion of gun cotton have a similar influence upon the changes which gunpowder may be made to undergo; and that, secondly, the effect of heat upon the water produced by the decomposition of gun cotton, which forms so important an element in the action of this explosive, has most probably its parallel, to no unimportant extent, in the vaporizing effect of heat upon the solids (especially upon sulphide of potassium) produced in the explosion of gunpowder. These are matters which demand their full share of consideration and investigation before it can be admitted that a sufficient explanation of the remarkable differences between the effects of gunpowder and gun cotton exists in the assumption that certain products of decomposition of the former must be regarded entirely as waste matter in the material, simply because they are solid *at ordinary temperatures*. The fact that gun cotton is *entirely* converted into gases and vapor *at the moment of ex-*

plosion constitutes unquestionably one of the great advantages which that substance possesses over gunpowder; but it is premature, at present, to assume, in comparing the action of the two substances, that only 32 per cent. (or even 60 per cent.) of gunpowder exist as gas or vapor *at the moment of its explosion.*

It is to be expected that the investigations which are now being actively pursued upon the true chemical effects produced in the explosion both of gun cotton and gunpowder, under conditions similar to those which attend their employment in practice, will aid materially in furnishing the correct data so essential for a thorough and impartial comparison of the nature and merits of these two explosive agents.

On the Supposed Nature of Air prior to the Discovery of Oxygen.

By GEORGE F. RODWELL, F.C.S.

From the London Chemical News, No. 245.

(Continued from vol. xlviii., page 408.)

7. *Discovery of Boyle's Law.*—About a year after the publication of Boyle's "Physico-Mechanical Experiments," Francis Linus published a treatise* in which he sought to prove the impossibility of the production of a vacuum, and the consequent fallacy of the results obtained by Boyle. The objections brought forward by Linus are, if possible, more absurd than those of Hobbes; they were very ably answered by Boyle,† and utterly unreasonable as was the theory on which they were based, we cannot but feel glad that it was propounded, inasmuch as in making experiments for its refutation, Boyle discovered the celebrated law of the compression of gases.

Linus admitted that the air possesses both weight and elasticity, but he denied that it is competent, in virtue of those properties, to produce the effects attributed to it by the vacuists.

One of the chief arguments adduced by Linus, against the explanation given by the vacuists of the Torricellian experiment is the following:—If we take a tube less than 29½ inches long, say 20 inches, fill it with mercury, and then invert it in a vessel of mercury, the upper orifice of the tube being closed by the finger, the latter is found to be strongly pressed down; now, he reasons if the air can support 29½ inches of mercury, and there are but 20 inches in the tube, the finger ought to be pressed upwards instead of downwards. To this Boyle answers very truly, that the finger has greater pressure on its upper surface than on its lower, because it has to bear the whole weight of the atmosphere on its upper surface, whereas below it has to bear the weight of the atmosphere — the weight of the column of mercury in the tube; hence, if there are 20 inches of mercury in the tube, the lower surface of the finger will be pressed upon by only one-third of the weight which presses upon its upper surface.

* "De Corporum Inseparabilitate, 1661."

† See "A Defence of the Doctrine touching the weight and spring of the air, proposed by Mr. Boyle in his new Physico-mechanical experiments, against the objection of Franciscus Linus." By the Author of those experiments. London: 1662.

Again, argues Linus, if a tube 20 inches long is nearly filled with mercury, the upper orifice closed by the finger, and the lower opened beneath the surface of mercury, the mercury in the tube will descend somewhat, and the finger will be pressed strongly against the tube, if the air, then, cannot support 20 inches of mercury, how can it support 29½ inches? And if it be argued that the spring of the included air pushes down the mercury, it follows that the finger ought to be pushed upwards; but, answers Boyle, the upper part of the finger has to bear the whole weight of the atmosphere, whereas the air in the tube beneath the finger has the weight of the column of mercury to help it to overcome the weight of the atmosphere.

In order to account for the suspension of the mercury in the Torricellian experiment, Linus proposed his "Funicular hypothesis," which affirmed that the mercury remains suspended in the tube by a "funiculus" or cord of very thin matter, which is forcibly distended by the weight of the mercury, and perpetually endeavors to contract; when the mercury column has distended it to the extent to which such a weight is competent, the column rests suspended in the tube by the funiculus. Not only the Torricellian experiment, but all the effects affirmed by the vacuists to be due to the weight and elasticity of the air were attributed by Linus to the action of his funiculus.

Boyle speaks of the theory as being "partly precarious, partly unintelligible, and partly insufficient, and besides needless;" it is curious, moreover, he remarks, that the funiculus should not only support a column of such a heavy liquid as mercury, but should also be able to draw in the sides of a glass vessel with sufficient force to break it; it is also remarkable that a pendulum should vibrate so readily in an exhausted receiver, if it has to cut through a number of stretched cords.

Pascal's Puy de Dôme* experiment proved decisively the fallacy of all the theories of the Plenists; Linus was well aware of this, and he chose to doubt the accuracy of the experiment, because having tried it on the summit of a hill of but small elevation, it did not appear to succeed. Boyle replies that inasmuch as the Puy de Dôme experiment had been repeated five separate times, and had also been made in England with similar results, there can be no doubt of its accuracy. Mr. Richard Townly, a friend of Boyle's modified the experiment by allowing a small quantity of air to remain in the tube above the mercury, and he found this enclosed air occupied a larger volume on the summit of a mountain than at its base.

In order to prove that the experiment obtains with small elevations, Boyle constructed a modified air thermometer, which would indicate very slight changes in the pressure of the air. This instrument consisted of a capacious glass bottle, into the neck of which a glass tube open at both ends was cemented; the tube reached nearly to the bottom of the vessel, and dipped beneath the surface of a small quantity of water placed therein, thus the air within the vessel had no communication with that outside; increase of pressure of the air would ob-

* For an account of this experiment see the third of these papers, *Chemical News*, vol. viii, p. 247.—Journal Frank. Ins. vol. xlviii, p. 171.

viously cause the water to sink within the tube; whereas diminution of pressure would cause the air within the instrument to expand, and consequently to raise the water in the tube above the level of that in the vessel; it is needless to remark that such an instrument would only show difference of pressure in the air when the temperature was constant. The exact height at which the water stood on the ground was observed. Boyle then drew up the instrument by a rope to the leads of Westminster Abbey, a height of seventy-five feet from the ground; the water was found to have risen one inch in the tube, the temperature remaining constant.

We come now to the discovery of the important law which affirms that *the volume of a gas varies inversely as the pressure to which the gas is submitted*, and inasmuch as there is some dispute as to whether Boyle or the French philosopher Mariotte was the first to discover the law, we will consider the subject somewhat in detail.

In France and Germany the law is almost universally called "the law of Mariotte," in England generally "the law of Boyle and Mariotte," sometimes "the law of Mariotte," seldom "the law of Boyle." I shall endeavor to prove that it is rightly the law of Boyle.

Roberval* enclosed a partially inflated carp's bladder in the Torricellian vacuum, and observed that it became fully inflated. Boyle made the same experiment with a lamb's bladder in the vacuum produced by his air-pump; on exhaustion, the bladder became fully inflated, and on the re-admission of air contracted to its former dimensions; he next inclosed air in a tube and measured its expansion on the removal of pressure; it was found to expand more and more with every stroke of the pump, until eventually it occupied 152 times its original bulk, to which latter it returned on the re-admission of air. Here, then, was the important fact that the greater the amount of pressure removed from air the more does it expand.

In order to see if the pressure of the air is the sole cause of the suspension of the mercury in the Torricellian experiment, Boyle (in the 17th of the Physico-mechanical experiments †) filled a tube three feet long with mercury and inverted it in a vessel of mercury, which latter he placed in the air-pump receiver, the tube being passed air-tight through its cover; on exhausting, the mercury fell lower and lower with every stroke of the pump, till it was almost on a level with the mercury in the vessel in which the lower end of the tube was placed; on admitting air into the receiver, the mercury rose above twenty-nine inches, on letting out the air it sank again to that height. It was thus indisputably proved that the height of the mercury is greater or less according as the density of the air pressing upon it is greater or less.

As the capacity of the receiver and of the air-pump cylinder could be readily determined, and thus the fall of the mercury in the tube due to

* This experiment is first mentioned in a work which was published by the Academia del Cimento, and which contains an account of the experiments made by its members. Of this important scientific society we shall have to speak hereafter.

† This and the previous experiment have been before mentioned in the fourth and fifth of these papers. They are introduced here because they bear upon the discovery of Boyle's law.

the removal of a known amount of air, Boyle hoped to be "enabled to give a near guess at the proportion of force betwixt the pressure of the air (according to its various states, as to density and rarefaction) and the gravity of quicksilver," but on account of several difficulties which he mentions, and of the little leisure at his disposal, he says he shall content himself with having suggested the experiment.

We see, therefore, that prior to 1660 Boyle suggested a method for determining the relation of the density of air to the weight which it supports. After making the experiments given above, the discovery of Boyle's law was inevitable; no thinking man could make those experiments without inquiring whether the removal of a definite amount of pressure caused a definite expansion of the released air.

Let us now pass on to the actual discovery of the law.

The fifth chapter of the second part of Boyle's "Defence against Linus," is entitled "Two new experiments touching the measure of the force of the spring of air compressed and dilated."

Linus, as before mentioned, had throughout his treatise affirmed that the elasticity of the air is incompetent to produce the effects attributed to it by the vacuists; the experiments detailed in this chapter were made by Boyle in order to prove that the spring of the air is able to effect not only as much but far more than the vacuists attribute to it.

To prove this Boyle procured a long glass tube, and bent it in the form of a syphon with parallel limbs, each of which was divided into inches and eighths; one limb was considerable shorter than the other, and its orifice was hermetically sealed,* (in fact, the instrument was exactly similar to that which we use in the present day for illustrating Boyle's law.) Mercury was poured into the instrument until it stood level in each limb, at the commencement of the scale, mercury was then poured into the longer limb until the air in the shorter was reduced to half its original bulk, "when," he writes, "we cast our eyes upon the longer leg of the glass on which we likewise pasted a list of paper carefully divided into inches and parts, and we observed, not without delight and satisfaction, that the quicksilver in that longer part of the tube was twenty-nine inches higher than the other." . . . "We were hindered," he continues, "from prosecuting the tryal at that time by the casual breaking of the tube. But because an accurate experiment of this nature would be of great importance to the doctrine of the spring of the air, *and has not yet been made (that I know) by any man*, . . . we at last procured a tube of the figure exprest in the scheme." This tube was exactly similar in form to the other, but larger in its dimensions; from experiments made with it Boyle deduced the following table which he calls

*Boyle had previously (in the 36th of the Physico-mechanical experiments) made use of a similarly shaped tube open at both ends for determining the relative weights of water and mercury. Mercury was poured into the tube till it stood level in each limb at the commencement of the scale; water was then poured into the longer limb and it was found that $30\frac{1}{4}\frac{1}{8}$ inches of water balanced $2\frac{1}{4}\frac{3}{5}$ inches of mercury, making the relative weights of mercury and water as 1 to $13\frac{23}{117}$.

"A Table of the Condensation of the Air."

A Space occupied by the air in the shorter limb of the instrument in inches.	*B* Height of the column of mercury in the longer limb, the weight of which column caused the change of volume shown in *A*.	*C* Pressure sustained by the air in the shorter limb expressed by the height of the column of mercury in the longer limb + 29 inches, the height of the column of mercury supported by the atmosphere.	*D* Theoretical pressure sustained by the air in the shorter limb, if we suppose "the pressures and expansions to be in reciprocal proportions.
12	00	29 2/16	29 2/16
11½	01 7/16	30 9/16	30 6/16
11	02 13/16	31 15/16	31 12/16
10½	04 6/16	33 8/16	33¼
10	06 3/16	35 5/16	35
9½	07 14/16	37	36 15/16
9	10 2/16	39 4/16	38 12/16
8½	12 8/16	41 10/16	41 2/7
8	15 1/16	44 3/16	43 11/16
7½	17 14/16	47 1/16	46 8/16
7	21 3/16	50 5/16	50
6½	25 2/16	54 4/16	53 10/16
6	29 14/16	58 15/16	58 2/16
5¾	32 2/16	61 4/16	60 8/16
5½	34 15/16	64 1/16	63 6/11
5¼	37 15/16	67 1/16	66¾
5	41 9/16	70 1/16	70
4¾	45	74 2/16	73 1/16
4½	48 12/16	77 14/16	77 3/8
4¼	53 1/16	82 3/16	82 4/17
4	58 4/16	87 7/16	87 3/8
3¾	63 12/16	93 1/16	93 1/8
3½	71 5/16	100 7/16	99 6/7
3¼	78 1/16	107 3/16	107 7/16
3	88 7/16	117 9/16	116 4/8

Boyle next made some experiments on the expansion of air under diminished pressure. He was informed, however, by the afore-mentioned Mr. Townley, that he (Mr. Townley), since reading the "Physico-Mechanical Experiments," had endeavored—acting, I suppose, on the suggestion given by Boyle in the seventeenth experiment mentioned above—to determine the amount of expansion of a certain quantity of air on the removal of known amounts of pressure. Townley had commenced a paper on the subject, which he showed to Boyle, but it was apparently never published.* Boyle does not mention whether he employed the same method as that adopted by Townley.

Boyle's method was the following:—A long tube, one end of which

* There are several papers by Townley in the early numbers of the *Philosophical Transactions*, but not one of them relates to the subject in question; neither do I find mention of any paper by Townley on the subject in Watt's "Bibliotheca Britannica."

was open and the other hermetically sealed, was divided into inches and eighths; it was filled with mercury and placed in a vessel of mercury of the same depth as itself. One inch of air was then passed into the upper part of the tube, the mercury within and without being brought to the same level. The tube was now raised till the air within it had expanded to double its volume, and the pressure acting upon it determined by subtracting the height of the column of mercury below the air from 29¾ inches, which at the time of making the experiment was the height of the column of mercury supported by the atmosphere. The arrangement of the apparatus was exactly the same as that figured in our present works on physics. Boyle gives a table of the results which he obtained, which he calls "A Table of the Rarefaction of the Air."

Let us now consider the claims of Mariotte.

*Fourteen years** after Boyle published the above experiments, Mariotte's "Essai sur la Nature de l'Air," first made its appearance. In this essay† Mariotte informs the reader that the first great property of the air is that it possesses weight, the second that it is capable of condensation and expansion; he then passes at once to the question whether the air is condensed in exact proportion to the weight which acts upon it. In order to ascertain this, Mariotte, together with a M. Hubin, procured a glass tube 40 inches long, sealed one end of it hermetically, and poured in mercury until it stood at 27½ inches from the end of the tube, the open end was then closed by the finger, the tube inverted, and opened beneath the surface of mercury; the mercury in the tube fell till it rested 14 inches above that in the vessel, and the air occupied 25 inches; 24 inches of mercury were next placed in the tube, on inverting the mercury fell to 7 inches, and the air occupied 32 inches; by these and other experiments, made with different quantities of mercury in the same tube, similar results were obtained. We may, therefore, writes Mariotte, take it "pour une régle certaine, ou loi de la nature, que l'air se condense à proportion des poids dont il est chargé."

The truth of this law he continues, may be more strikingly proved by taking an inverted syphon tube, one limb of which is 8 feet long and the other 1 foot, &c. He here proceeds to describe the construction and manner of using the instrument we have before mentioned as being that by the aid of which Boyle discovered the law.

It appears almost inconceivable that in sixteen years Mariotte should not have read the "Physico-Mechanical Experiments," in which we have seen Boyle so clearly suggested experiments on the subject in question, and it appears almost as inconceivable that in fourteen

* The author of the article on Mariotte in the "Biographie Universelle," (1820) writes—"Son discours sur l'air, qui parut en 1679 renferme une suite d'expériences intéressantes, alors àbsolument neuves." The "Nouvelle Biographie Générale," (1860,) on the other hand, states that the essay appeared in 1676. In the preface to Mariotte's collected works it is stated that the essays "On Plants," "On the Air" and "On Heat and Cold" appeared between 1676 and 1679. In the text I have given Mariotte the benefit of the earlier date.

† See "Oevres de M. Mariotte de la Academie Royale des Sciences." Paris, 1740.

years* he should not have read the "Defence against Linus," containing not only the discovery of the law, but more elaborate experiments than he gives to illustrate and prove it. I find, however, no mention of Boyle's experiments in Mariotte's essay, at the same time he appears to be relating known facts rather than perfectly new and original experiments; it is curious, moreover, if he knew nothing of Boyle's experiments, that he should have used the same instrument, and directed it to be made of the same dimensions as those recommended by Boyle.

But allowing that Mariotte discovered the law independently of Boyle, is priority of discovery nothing? Do we not know that it is everything in the present day? And if now why not two hundred years ago? Suppose a man were to discover a metal possessing as he believed, properties different from those of any known metal, that he published an account of it, and that it proved to be cæsium, should we, in speaking of the discovery of the latter, couple his name with Bunsen's? Most assuredly not, however laboriously he had worked, however elaborately his research had been carried out. It is the duty of every man, before commencing a research, to find out how much has been done by other experimenters; if this were done more completely, what a large amount of controversial matter would be excluded from scientific literature.

It is true that 200 years ago there were less opportunity of ascertaining the results obtained by other experimenters than in the present day; there was so little physical science in the world, Nature had for so short a time been interrogated by experiments, that many philosophers had a habit of believing everything they discovered was new, and did not trouble themselves to ascertain the results obtained by others, but surely in fourteen years Mariotte might have made himself thoroughly acquainted with all the important discoveries which had been made relative to the air.

I trust, on consideration of the above facts, it will be conceded that the law which affirms that the volume of a gas varies inversely as the pressure to which the gas is submitted, is not "the law of Mariotte," but "the law of Boyle.

* It will be remembered that the "Physico-Mechanical Experiments" appeared in 1660, and the "Defence against Linus" in 1662.

(To be continued.)

Atmospheric Pressure as a Source of Mechanical Power.

To the Editor of the Journal of the Franklin Institute.

I am with your correspondent E. in his ideas and hopes of a popular motor from the atmosphere, assured as I am that seeking for it is not fishing for a one-eyed perch. In fact, the question is not so much, is it to be had, as what will it cost.

The subject of the forces, like that of natural history, extends to all things on the earth's surface and in her interior. They are represented as constituting two species, organic and inorganic; the former

containing two distinct groups, animate and inanimate; and the whole pervading the mineral, vegetable and animal kingdoms. But, while thus diversified in their forms and functions, they are essentially one. That which animates a living organism is not itself organic. There are but two marked divisions, *mechanical* and *chemical*, and they are recognised on the ground of conventional use and facility of description and research.

Mechanical principles are limited and tolerably well understood, but while the theatres of their operations will become more and more enlarged, there is an increasing appeal to chemical science, and from it, doubtless the great novelties of the future are chiefly to come. In turning from physical to chemical forces, it is not to be supposed that the latter are more valuable *per se* than the former. In that respect all are alike, the effect of a certain amount of heat being the same, however evolved and applied—through aerjform, liquid or solid media. In nature's store-house all are labelled at one price, hence the advantages to be derived from one over another, arise from its better adaptation to some particular class of movements, to locations where the requisite materials are accessible, to where society is prepared for and in want of it.

We have no means to create force but only to transfer it from one body to another; and, as everything here is an embodiment of it, our common motors are first taken from those that readily yield it, as animals, wind, and running or falling water; next by changing the form of bodies, as solids into liquids and gases, and liquids into vapors. This is done by means of heat, which is not only a form but the primary form of terrestrial force; and, we should say, the constituent element of direct or positive force in every concrete sphere. Such spheres are the results of gravitation; that is, of condensation, while heat is the product of condensation and the universal agent of expansion. To diminish it in any body is to evolve a negative or contracting force. Deprived of heat in one form, it becomes an absorbent of it in another.

Of sources of mechanical power, there are none like the weight or pressure of the atmosphere in ubiquity or uniformity. Others are variable, some flitting and intermittent, most are local and some give out, but it never fails. In all climates, seasons and places, in all weathers, at all hours, it promptly responds to every call and will do so as long as the earth endures. Nor are we sure that there are any limits to its capacity as a mover of useful mechanism. All the demands that man can make on it, were they multiplied indefinitely, cannot affect its general or special functions in the economy of the planet an iota. Its power, like that of nature, is omnipresent and invariable, silent and invisible, bland and beneficent. It is not, however, likely to be fully appreciated for some time to come. There are not many who believe that this aerial envelope, which modifies solar heat and light, without which sound and music would be unknown, the vitalizing element of the vegetable and animal creation, which ministers to all our senses and sustains life, is competent to heighten its enjoyments by relieving us from physical drudgeries.

It has lately gone further and added a remarkable distinction between us and the tribes below us. They move in air and water by exerting their propelling organs against them, while we make the element we breathe act as a force to convey us through it without any muscular effort at all. Most certain it is, that a common force from the atmosphere, however and whenever obtained, will be hailed by the world as a worthy companion of steam. As chemistry must have a hand in it, it would be a brilliant addition to electrotype, the telegraph and photography.

There is a prevailing impression that atmospheric prime movers are hardly worth mentioning since Newcomen's time; that to commend them is to give weight to smoke, importance to trifles. Perhaps so, but let us look before leaping to that conclusion. Low steam then used to produce a vacuum became superseded by the expansive force of the vapor in the popular and justly popular high pressure engine. It is, however, consistent with this that atmospheric pressure may have work to do, appropriate to it without infringing on, or depreciating other motors. To those who have not made themselves familiar with the phenomena of steam, there is a fact bearing on this of very marked significance, viz: that the mechanical value of, or force to be derived from, steam is very nearly the same, under whatever temperature or pressure it be raised. This could not have been anticipated by practical men. To many it may still appear incredible that steam propelling a train of cars possess little, or no more value than the same quantity issuing from a tea-kettle spout, or from under the cover of a common pot.

The difference between the condensation of low, and the expansive force of high steam is thus given by Brande, as recognised results of English experiments: "To use round numbers, it may then be stated that by the evaporation of a cubic inch of water, a mechanical force is produced equivalent to a ton weight raised a foot high; and that this force is very near the same, whatever be the temperature or pressure under which the evaporation takes place." The following items from the voluminous table in the appendix to Lardner's work on the steam engine afford a sufficient explanation.

	Pressure in lbs. per sq. inch.	Corresponding Temperature.	Volumes of steam to that of water.	Mechanical effect of a cubic inch of water evaporated in lbs. raised 1 foot.
	1	102°1	20868	1739
	2	126°3	10874	1812
	3	141°6	7437	1859
Pressure of the atmosphere,	15	212°0	1700	2086
1 lb. on the safety valve,	16	216°3	1573	2097
2 lbs.	17	219°6	1488	2107
15 "	30	251°6	883	2209
35 "	50	283°2	554	2305
85 "	100	332°0	295	2462
185 "	200	387°3	158	2636

I have added the left hand column because there are those who do not perceive that there is any greater pressure in a boiler than what the safety valve indicates.

Without demonstrations like these it might have been thought that the mechanical value of a definite quantity of steam increased with its tension: and so it would if its volume did not diminish with the pressure. Thus, under 1 lb. on the square inch its volume is over twenty thousand times that of the water from which it is evolved; at 2 lbs. it shrinks into almost half that; at 15 lbs. into one twelfth; at 50 lbs. into a thirty-seventh; at 100 lbs. into nearly a seventieth; at 200 lbs. into a hundred and thirty-seventh.

Who could have foreseen that the work done by steam under a pressure of 200 lbs. on the inch is not double that of the fluid under 1 lb. in truth only half as much more. There are other attending singularities; the difference is extreme between using it to produce a vacuum under a piston and as a pushing force against it, yet how surprisingly alike the results. A high pressure engine acts against the atmosphere; against a force of 15 lbs. on the inch, and this force is all that the other plan has; that which one rejects as a trifle and pushes aside as a nuisance constitutes the capital of the other, and a capital that yields an interest nearly equal to that of its popular opponent.

No better way of exciting and employing atmospheric pressure as a physical force has been devised than producing the vacuum under a piston, by admitting some substance beneath it while rising and expelling it as soon as the cylinder is filled. Fluid bodies are alone adapted for this, and, of permanent gases common air is the readiest and cheapest. Every where present it enters without trouble or cost. But how is it to be withdrawn? Not by mechanical force since more would be required than the vacuum could give in return, and no means have been discovered to condense it into a liquid. Recourse has been had to explosive gases and compounds, but they, besides being too costly, are in other respects inconvenient. They heat the cylinder, and what is worse, clog the piston. This might be avoided by using the blast somewhat prolonged, outside of the cylinder on the principle exemplified in the paper on increasing the blasts of bellows, published in the Journal for May, 1858. If gun cotton could be thus successfully employed, it would give us the neatest and cleanest of portable forces. See also Ewbank's Hydraulics 489-491 for vacua of over 20 inches of mercury by currents of steam.

Another mode is exhibited in the old experiment of holding a slip of lighted paper under an inverted tea cup and dropping it into a saucer of water, or as air is driven out of a cupping glass by a piece of cotton dipped in alcohol. There can be little difficulty in applying the principle to an engine, and if petroleum oils are going to be as abundant as appearances promise, an available force, with some modifications of the apparatus may be obtained.

But vapors that resume the liquid form at common temperatures are deemed the most promising. Liquids which boil at low degrees of heat would seem the best, but there are other conditions to be observed, as

the volume, density, and cost of the vapor. The three liquids in the annexed table exhibit some striking differences. The first can be had for nothing, the second for from two to three dollars a gallon—before the war from 50 to 75 cents—while the price of the last ranges too high at present to be thought of.

Liquids.	Boiling Point.	Volumes of Vapor to one of Liquid.	Density of the vapor, air being 1.
Water,	212°	1700	625
Alcohol,	176°	660	1613
Ether,	96°	443	2586

The density of alcohol being more than 2½ times that of steam, if it would answer to charge a cylinder only half full and that were to expand and adequately fill it, something might be gained; the liquid, of course, being used over and over without waste. It is doubtful, however, that water can at present be superseded. Evaporating at ordinary temperatures, it boils at 212°. For the purpose in question, no higher temperature is wanted. Rather a lower one, if possible; and, as ebullition does not affect the nature of steam, but simply increases its evolution, it is worth while to inquire if that cannot be done to some extent, mechanically, by agitation for example, as when cooks stir up the contents of their caldrons.

A large portion of the early formed globules or vesicles never reach the surface of the water, being ruptured or condensed on their way up. They are being evolved when a kettle is said to simmer or sing, but not till the whole of the liquid is heated to 212° do they rise without, more or less, being condensed. It has been found that when a vessel has just stopped boiling, if little angular pieces of metal be dropped in, ebullition is renewed. The points afford bases for and facilitate their formation, hence it may, I think, be taken for granted that the suspension in the water of a mass of metallic spiculæ, or other jagged bodies, would materially hasten their evolution and ascent. It would tend to supply a cylinder with saturated vapor, that which is most readily condensed, and of the lowest temperature. But whatever the latter might be I would cut off the supply before the piston reached the top of the cylinder. The small force thus expended, would be made up by the attenuation of the fluid, a less amount of heat to be abstracted and a quicker condensation. Would it not be an advantage to make the piston thus initiate the vacuum; predisposing, as it were the vapor for collapsion? To accomplish that with the smallest quantity of the cooling agent, I would make the parts on which the steam acts as thin and light as possible.

Watt was so embarrassed and baffled by the amount of metal in his cylinders and pistons, bed-plates and side pipes, that he was driven to effect condensation in a separate vessel. The first steam engines were

exceedingly massive. Such was the one he made for Fulton's first boat. With it I was familiar, while it remained in the factory at Powless Hook after his death. It is no wonder that Watt became, as we are told, forcibly impressed with the waste and expense of steam and fuel in the operation of condensation. In the theory of the atmospheric engine, one cylinderful of steam should suffice for each stroke of the piston, whereas he found the actual consumption was four to five cylinderfuls per stroke. Nor was this all. On examination he discovered the source of this waste to arise from the necessity in order to make the piston ascend, not only to condense the steam *but to cool the whole mass of the cylinder down to* 100°, and to make the piston descend it was necessary not only to fill the cylinder with steam, but to *raise the temperature of cylinder and piston from* 100° *to* 212°.

He had other obstacles to overcome. His engine was so far in advance of the mechanical capabilities of the age that it was with the greatest difficulty it could be executed. Smeaton thought it could never be introduced into general use on that account. The cylinders were often wider at one end than the other. Speaking of an 18-inch one, Watt says, in a letter, that the longest diameter exceeded the short one $\frac{3}{8}$ of an inch! It was not till then that the revolution in working metals began by a revolution in tools. There were no self-acting ones. Slide rests and planing machines unknown; everything being executed by hand. See Smiles' "Industrial Biography: Iron workers and tool makers."

Watt got rid of his condensing troubles but it was at the expense of additional machinery and a large outlay of force to work it. According to Lardner *four-tenths* of the mechanical force of both condensing and non-condensing steam engines are intercepted and consumed by them, leaving only six-tenths available. He enumerates friction and cooling of the steam in passing to the cylinder, radiation of heat from it and its appendages, friction of the piston, loss by leakage, force expended on expelling the steam after its action on the piston, force to open and close the several valves, to pump up water for condensation, and to work the air-pump. It seems scarcely possible that an atmospheric engine can intercept 14, much less 40 per cent. of its power.

Qy. What did Watt gain in power, by his separate condenser? Was not the expense of working it equal to the three or four cylinderfuls of steam consumed on the previous plan?

As for agents of condensation it is not easy to suggest anything new. I once thought of naming, with the view of enlarging the range of inquiry, jets of an hydrous vaporsor liquids to induce coalescence of the globules, or puffs of some deliquescent powders to hasten liquefaction. Then the idea that possibly something might be added to water to render it more readily evaporable. If we had ample means promptly to liquefy aqueous vapor, there would be no occasion to be so particular about keeping down its temperature, and that such are to be found in Nature's laboratory, it is said, there is no doubt as the most

ardent inquirers have but barely entered it—that of her resources nothing is known compared to what is to be known—that chemistry progressing we shall have as cheap a source of cold as we have of heat, and that *then* atmospheric motors may become common.

The problem is then one of heat and cold—alternate changes of one to the other. Were this all we should have only to wait till the atmosphere abstracted the heat through the sides of the cylinder, but the operation would be too slow, hence, time is an element in the solution. Whatever number of downward strokes the piston is required to make per minute, just so many times must the steam enter and be chilled back into water. Now, before appealing to, or waiting for, chemists, if we look out on what nature has done already, we shall have to take to ourselves a rebuke addressed to another people. "Ye see indeed but perceive not." She has established a periodical frigorific supply almost as abundant as water, and with no greater tax on it. It is offered us *ad libitum* and for nothing. We cannot have anything cheaper than steam to charge a cylinder, and what is there in greater plenty in these latitudes than *ice* to condense it. During the summer it has been and is now (October), being delivered at our houses for 30 cents per 100 lbs. By the quantity it may, we presume, be laid up at little over a dollar per ton, in some places for less. We have thus in the opposite conditions of one substance, and readily convertible into each other, both the desired elements for exciting atmospheric pressure, and that substance gratuitous and most profusely differ. What more can be hoped for? It is folly to wait for something better than ice, and as for science ever furnishing anything equal to it on the same terms, it is worse than folly to expect.

It perhaps will be thought, if there is such virtue in ice it would have been brought forward long ago and be in extensive use now. That does not follow. Innumerable things are obvious to sight, the future uses of which are not discerned. The case is one of those which show how in the arts one thing is the sequence of another. Ice has only lately become an article of merchandise. But for the fact that as regular a trade in it has been established as in coal and other things of daily consumption, it had probably not yet been thought of as an element in the generation of force.

The amount of heat necessary to evaporate a certain quantity of water is about the same under all pressures. The difference lies here between a steam engine and an atmospheric one;—the former throws into the air at every stroke the hot contents of the cylinder. According to some writers 90 per cent. of the heat and consequently of the fuel is wasted—not 10 per cent. utilized. Hence a continual flow of fresh water into the boiler. With the latter the water is merely kept boiling as in an open vessel and the condensed steam runs back into it at a temperature of 100°. There is no discharge of waste heat except what the ice and ice-water takes up. The amount of fuel is therefore nominal compared to that of a steam engine, and so is the size of the boiler. One little larger than a domestic caldron should suffice for one or two horse power.

The quantity of water required for the boiler is readily determined.

Suppose the working cylinder of the capacity of a cubic foot, the area of the piston 144 inches, and a 12-inch stroke. The pressure on the piston would be 2160 lbs. and this moving at the rate of a fraction over 15 feet per minute would reach the standard of horse power; so that 15 cubic inches of water—less than half a pint—would have to be evaporated every minute for each horse power.

The quantity for liquefaction of the steam is a great deal more. If a cubic inch of water in the form of steam at 212° be introduced into the same vessel with 5½ cubic inches of ice-cold water, the result will be 6½ cubic inches of water at 212°. Hence steam at 212°, requires 5½ times the water in it to reduce it into water at the boiling point, and an additional quantity to reduce it to 100°. It is found in practice, says the Encyc. Britannica, that the most economical vacuum for English Steam Condensing Engines varies, according to the pressure from 95° to 105° equal to 26 and 27½ inches of mercury when the barometer is at 29½ inches, and that not less than twenty times the water in the steam is required to condense it. This large quantity—20 cubic inches to a cubic foot—is, however, used at the common temperature of 50°. If atmospheric engines required so much we should have but feeble hopes of them.

To sustain a proposition in favor of ice as a condensing agent something more will be expected than its profusion and little cost. To secure its adoption it should be more efficacious than water and a less quantity required. The following facts leave no room for doubt on these points. The singular difference in the cooling power of water in its liquid and solid forms is of course accounted for by a less amount of latent heat in one state than in the other.

If one pound of water at 174° be added to another pound at 32°, the result is two pounds at 87° (just half of 174°). But a pound of water at 174° added to a pound of ice produces two pounds at 32°. That is, ice-cold water lowers the temperature 87 and ice 142°. To test this I have taken advantage of the presence of snow. One day, the air at 36° and the snow soft, a pound of water at 32° and another at 174° gave two pounds at 100°, while snow in place of cold water reduced the mixture to 44°. Another day to 36°, the snow being hard and icy. The temperature of a pound of boiling water and another at 32° was 108°. With snow in place of the cold water it was 36°. The mean of three experiments with half a pound of icy snow to a pound of boiling water was 98°.

Such is the difference between using ice and ice-cold water, but as jets of condensing engines, given at the common temperature of 50°, instead of 32° the difference is still greater. Allowing a liberal margin, may it not be inferred that one-third of water in ice and snow will be as effective in atmospheric as three-thirds of the liquids are in common condensing engines.

It will perhaps be objected that to apply a solid body like ice, or a semi-solid one like snow in the manner of a jet is not to be easily done. True, but showers of pulverized ice are not impossible things in the face of their acknowledged superior effects.

Modes of applying the cooling agent:—Could an external application of it be made to answer, there would be no waste of water from the boiler and no trouble with it after being once charged. The process might apply to alcohol and ether but hardly to steam at present. Still it may be among the possibilities.

An internal application necessitates a discharge of warm water from the cylinder at every descent of the piston except what is returned to the boiler. Of two modes of internal condensing, a descending shower through a piston made hollow and the lower plate finely perforated (as in an experimental model) and a jet from below, the first has its advantages but the latter is perhaps the most efficient, as it rises and falls in the vapor and therefore has a better chance of allowing less to escape contact with it. With a descending shower cylinders would have to be vertical and all things considered, that seems the best position for them; still circumstances may require them to be used horizontally.

As every machine is a vehicle of force, that which it absorbs is so much deducted from its available power. Thus the heat taken up by the cylinder and piston is, as already seen, a clear loss in itself and incurs an additional one in ice to abstract it. It would be well to make the acting parts of, or to case them with, a material that absorbs neither heat nor moisture, that there may be nothing but the steam to be cooled. Large syringes are now made of vulcanized rubber and bear a temperature of between 300° and 400°.

Steam, like other motive agents, has no means within itself to increase or diminish its energy, hence we add fuel to a boiler as we add power to a lever, and it remains for experiment to determine the comparative value of high pressure and atmospheric engines by ascertaining whether the additional fuel and accessories with resistance to the atmosphere, in one class, be less or more than the ice and its accessories with no resistance from the atmosphere in the other. But on whichever side the cost preponderates, or if on neither, there are, and always will be occasions, circumstances, or places, where one should have the preference. This is a provision of nature that applies to all forces.

High pressure engines waste more heat than others but they are preferred as common motors because they possess advantages that pay for the waste. They are more compact in their working parts and more energetic than atmospheric ones of the same dimensions can ever be. They will continue special favorites where fuel abounds. Among openings for others—Agriculture presents one. It wants a movable engine not exceeding half the weight and cost of a steam locomotive. An atmospheric one would be an invaluable acquisition to planters.

And what objections can be urged against placing the mechanism on city railway cars? None as to safety since explosion of the small boilers would be impossible. The ruling consideration is economy. The working cost will be in the ice and coal. The amount consumed of the latter for each car during 12 hours will be comparatively small, and as for the ice, supposing a car use up two tons in that time its

cost would not exceed three or four dollars at most. It would of course, be taken up in parcels every two or three trips like a steam locomotive taking in water, of which they would require none.

In high latitudes where firing materials are few and sparse, atmospheric motors may become common, while steam will be the general one in temperate and tropical zones and wherever fuel is plenteous. I see no improbability in ice becoming one of the means, as an agent of force, of equalizing the extremes of national conditions.

Physical force is indispensable to social progress. As long as men have none but their own they cannot rise out of barbarism. Those of the Arctic regions would seem to be the last to possess inorganic forces, but when they learn that ice can be used as an adjunct of heat in their evolution, they will have a still higher estimate of their great staple of it and snow, and furnish another illustration of the principle of compensation—so remarkable in the organization of animals but which extends to climates, their products and people. *

(To be Continued.)

Exposition of the late Researches on the Cohesion Figures of Liquids.

By THOMAS THETHILL WRIGHT, M. D.

From the Journal of the Society of Arts, No. 631.

Dr. Wright stated that, on being requested to give one of the "Special Discourses to the Society of Arts," he had chosen the subject of the "Cohesion Figures of Fluids," as he was desirous of bringing before the practical men assembled at the meeting the remarkable researches of Tomlinson, as well as observations made by himself in the same field. The lecturer then proceeded to give a very full exposition of Tomlinson's discoveries, with numerous carefully executed drawings of the cohesion figures of various oils and other fluids when dropped upon surfaces of water and other liquids, figures which have already been exhibited by their discoverer at the Society of Arts in London, in his paper read before it. Dr. Wright then proceeded to describe two new classes of cohesion figures discovered by himself:—1st. Figures produced on perfectly clear and freshly split surfaces of mica; and 2d. Figures produced by drops of fluid on surfaces of mica or glass, when connected with the poles of an induction coil. He showed by the oxy-hydrogen lantern, on a screen twelve feet in diameter, images of both of these classes of figures, as well as numerous drawings of microscopic figures which were too small to appear on the screen. The mica figures were produced by placing a large drop of fluid, (sulphuric acid, nitric acid, tinctures of iodine, henbane, vegetable infusions, and a great variety of animal fluid and mixtures of them) on the surface of the mica, and gently breathing on them, when the cohesion figures instantly developed themselves in an infinite variety of forms. Those shown on the screen consisted of an infusion of the extracts of belladonna, senna, and hemlock, and of healthy urine, urine containing bile, and urine containing albumen. These figures were rendered visible, developed as it were, by dusting them with a powder

puff containing hair powder or the finest lampblack. Dr. Wright pointed out that all these figures presented a distinctly bisymmetrical form, and imitated, with remarkable clearness, the forms of various microscopic algæ and diatoms; while some of the images of urine, developed with carbon, in addition to their branching forms, were minutely veined, so as to resemble the finest specimens of agate. The second class of figures—the electric cohesion of figures*—differed entirely with the fluid used, the surface on which it was placed (whether glass or mica), the intensity of the electric current, and the pole (whether positive or negative) by which the fluid was electrized. Very splendid and large figures were made on chemically clean plate-glass by powerful induction coils. But the greatest variety of figures occurred when the thinnest microscopic glass and mica were employed, with a small induction coil, giving a spark of about a tenth of an inch. The electric figures on mica differed entirely from those on glass; and those on mica freshly split from those on mica washed, heated or exposed to the air; in fact the slightest change in the character of the fluid, or the surface on which it was dropped, resulted in the production of difference of figures. The electric figures projected on the screen imitated curious forms and species of Ulva, Ceramium, Delesseria, and finely branched lycopodiums. Dr. Wright stated that in these cohesive figures of Tomlinson and himself, the pattern designer possessed a source of infinite diversity of form, free from the stiffness of the designs furnished by the kaleidescope of Brewster. Many of the figures in mica glowed with all the gorgeous tints of the soap bubble.

At the close of the discourse, his grace the Duke of Argyll moved a special vote of thanks to Dr. Wright for his admirable exposition, which was seconded by Dr. Stevenson Macadam, and unanimously agreed to.

Pro. Roy. Scottish Soc. of Arts.

* In producing the electric cohesion figures, a plate of brass is laid on the table, and covered with a sheet of black paper dripping in a solution of chloride of calcium; on this the glass plate is laid, and in the centre of the glass plate the minute drop of fluid to be electrized. One pole of the induction coil is connected with the brass plate, the other with the drop. The figures instantly shoot out as soon as the induction coil break is set in motion.

Bituminized Paper Pipes, and Roof Sheeting.

From the Practical Mechanics' Journal, November, 1864.

These pipes are made on a cylinder of which the diameter is the required bore of the pipe; the paper is rolled in widths equal to the required length of the pipe, and passes through molten bitumen, until the required thickness of pipe is attained. Acting in connexion with this, another cylinder revolves against the first cylinder bearing the bituminized paper, the latter being thus subjected to a very great pressure, which causes a regular distribution of bitumen. When the pipe is taken off the cylinder, the inner surface is coated with an indissoluble water-tight composition, and the outer surface is coated with

bitumen mixed with sand. The compressed paper, without the molten bitumen forms one-third of thickness of the pipe. The great advantage of these pipes consists in their *resistance to internal and external pressure.* Their power of resistance is sufficient to withstand a pressure of more than 240 pounds to the square inch, or 15 *atmospheres*, equal to more than the pressure of a column of water 500 feet in height. In Hanover and Paris, where experiments were made with these pipes in reference to their resisting power, they withstood a pressure at the former city of more than 24 *atmospheres*, equal to about a column of water 800 feet in height, and in the latter city they withstood a pressure of more than 500 pounds on the square inch, equal to about a column of water 1000 *feet in height.* In making these trials not only one pipe, but several joined together were tested. As above stated the resisting power of the pipes can be considerably augmented by adding to the thickness. The manufacturers undertake to furnish pipes to resist a pressure of 600 pounds on the square inch. Judging from the experiments made by Mr. Samuel Hughes, Civil Engineer of London, and others, the resistance to external pressure of the bituminized pipes is so great that they can with perfect safety be laid under any depth of causeway, and bear equal or unequal pressure.

Durability.—The known property of asphalt to resist atmospheric changes, renders it most valuable for pavements and roofing, whilst when applied to pipes, exposed as a rule to the humidity of the soil only, it must augment their durability to an almost unlimited degree. Iron pipes are nearly always coated with asphalt to increase their durability and protect them, but the paper pipes, being composed in a great measure, throughout their substance of this material, must have a great advantage over them. More than ten years experience shows, besides, that paper pipes that were laid in Paris in the year 1851, when taken up were found to be in the same state as when laid down. As these pipes do not suffer either from concussion or frost, and are not liable to leakage or oxidation, (the causes of destruction to metal pipes,) their durability must needs be almost unlimited. *Impermeability.*—By the above described mode of manufacturing, a perfect homogeneous substance is attained, and the density of the bituminized material is greater than that of all other kinds of pipes. *Bitumen a Bad Conductor.*—Bitumen being a bad conductor protects the water in these pipes from excessive cold in winter, and in summer from heat; this same peculiarity farther leaves the length of the pipes unaltered by the change of temperature; this is not the case with metal pipes. *The inoxidability* of the bituminized paper pipes is another circumstance greatly in favor of their use as a substitute for iron tubes. Such inconveniences as destruction or obstruction by oxide, noxious chemical alterations of water, as in the case of leaden water pipes, impregnation of the water with oxide of iron, so very injurious for many purposes cannot take place with the use of tubes made from bituminized paper, neither is there the most trifling impurity imparted to the water passing through them.—*Bitumen unaffected by Acids and Alkalies.*—The bitumen pipes are not affected by acids or alkalies, and remain per-

fectly unimpaired when water, impregnated with acids or alkalies, is led through them. Metal pipes, on the contrary, are subject to rapid decomposition when placed in sulphuric acid, chalk soil, or copperas in solution, frequently found in coal-pits and mines.—*Non-conductor of Electricity.*—Through the peculiarity of bitumen not being acted upon by acids, and being a non-conductor of electricity, the bituminized pipes are not liable to destruction and stoppage, as iron pipes are in which galvanic streams are frequently produced, causing leakage and oxidization. In virtue of this peculiarity, asphalt pipes will be used with advantage for underground telegraph wires, through tunnels, and under bridges.—*Elasticity.*—The elasticity of these pipes protects the same from bursting by external shocks or concussions, which causes most of the other kinds of pipes to break, the capability, also, of extension is greater than the expansion caused by frozen water, hence bursting is avoided, an accident to which all other pipes are liable.—*Specific Gravity.*--The specific gravity of these pipes, compared to iron pipes, is as 1 to 5, which circumstance, as it facilitates transport and adjustment, considerably reduces the cost.—*Cheapness.*—In great Britain, where all kinds of metal pipes are undoubtedly manufactured cheaper than anywhere else, the bitumen pipes, when laid, are still only one-fourth the cost of lead, and about one-half the cost of iron. On account of their great lightness, the rate for transport is comparatively very small—say only one-third of the cost of transport for iron pipes. In conclusion, it may be mentioned, that all workings, repairs, &c., of bituminized pipes are much easier executed and at less expense than is the case with other pipes. The following bituminized pipes are manufactured in seven feet lengths. *A.* Water pipes to withstand the pressure of 15 atmospheres, either with bituminized couplings or with iron flanges. *B.* Gas pipes (lined with metal to resist the chemical influence of gas) with iron flanges. *C.* Air pipes for mining purposes, of lighter construction, and connected by bituminized couplings.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, February 16th, 1865.

John H. Towne, Vice President in the chair.

The minutes of the last meeting were read and approved.

The Board of Managers reported donations to the Library from

The Royal Astronomical Society, and the Society of Arts, London; the Canadian Institute Toronto, and Major L. A. Huguet-Latour, Montreal, Canada; Prof. A. Dallas Bache, and Frederick Emmerick, Esq., Washington, D.C.; the Legislature of Pennsylvania, Harrisburg, and Prof. John F. Frazer, Philadelphia.

The Special Committees on Steam Expansion; and on Weights, Measures, and Coinage of the United States, reported progress, and the Institute having been notified of the resignation of Mr. Briggs,

chairman of the last mentioned Committee, the Secretary was by motion directed to call the said Committee together. The Special Committee on an Uniform System of Screw Threads, &c., reported:—

That they had distributed 250 copies of their former report, accompanied by a circular letter and copy of Mr. Wm. Sellers' original paper upon the subject; to the individuals, companies, and government officials, interested in the matter, and anticipated soon to receive communications in return. Some remarks were then made upon the report of the Committee by Mr. Caleb P. Jones, relative to the diameter of nuts, and further consideration of the subject was then deferred until the next meeting.

The Special Committee to draft petition to the City Councils relative to the appointment of inspectors of Steam Boilers in the City of Philadelphia, reported progress.

The following Committees were then appointed by the President:—

On the Library.

~~Henry Ames,~~
Pliny E. Chase,
~~James H. Cresson,~~
James Dougherty,
~~John Ferguson,~~
~~George Harding,~~
~~John Hoskins,~~
~~James T. Lukens,~~
Henry Morton,
Henry G. Morris.

On Arts and Manufactures.

William Adamson,
John H. Cooper,
Charles G. Crane,
Henry R. Lawrence,
C. Eugene Meyer,
Jacob Naylor,
Isidore Osorio,
Percival Roberts,
William G. Rhoads,
Samuel S. White.

On Minerals.

Clarence S. Bement,
Isaac H. Conrad,
John F. Frazer,
Emile Geyelin,
~~J. Peter Lesley,~~
Robert H. Lamborn,
Robert E. Rogers,
~~Colman Sellers,~~
Richard A. Tilghman,
Elias Wildman.

On Exhibitions.

John C. Cresson,
~~James H. Cresson,~~
John Gardiner, Jr.
Edwin Greble,
~~Bloomfield H. Moore,~~
J. Vaughan Merrick,
William A. Mitchell,
T. Morris Perot,
~~John H. Towne,~~
O. Howard Wilson.

On Models.

James Agnew,
William B. Bement,
Edward Brown,
Charles H. Cramp,
Mordecai H. Haines,
Addison Hutton,
John Kile,
W. Barnet Le Van,
John L. Perkins,
S. Lloyd Wiegand.

On Meteorology.

Charles M. Cresson,
John F. Frazer,
Henry Hartshorne,
Caleb S. Hallowell,
James A. Kirkpatrick,
Henry Morton,
James A. Meigs,
~~Fairman Rogers,~~
~~James S. Whitney,~~
Samuel S. White.

On Meetings.

B. Henry Bartol,
Henry Cartwright,
John F. Frazer,
~~Henry Howson,~~
Edward Longstreth,
J. Vaughan Merrick,
Henry Morton,
Fairman Rogers,
Coleman Sellers,
Thomas Shaw.

On Scientific Proceedings of the Institute.

John F. Frazer,
J. Vaughan Merrick,
Henry Morton,
Fairman Rogers,
Coleman Sellers.

The Secretary then read his report on new inventions and discoveries. In this reference was first made under the head of *Mechanics*, to various machines lately introduced for the purpose of boring deep wells for water or oil; for cutting or digging coal by steam power; and for pumping or otherwise raising fluid material from great depths. Under the head of *Physics*, reference was made to certain improvements in tempering of steel, by the use of a highly conducting bath, as for example one of mercury, by which a higher degree of hardness was attained (with less intense heating, and consequent warping), than in the usual method. A series of tubes filled with various gases and vapors, exhausted to a certain point of rarefaction and then hermetically sealed, were then exhibited, with the Ruhmkorff coil. The flashes of statical electricity developed by that instrument being caused to pass through these tubes, (from platina wires melted into their ends when they were first being prepared), developed in them most brilliant flashes, streams, and clouds of colored light, crimson violet and blue, crossed in many cases by transverse dark bands, while the substance of the tubes themselves became luminous by *fluorescence* with tints of blue, yellow, or green, according as the glass contained, potash, soda, or a mixture of both.

One of these tubes also exhibited in a most striking manner the property of phosphorescence, remaining luminous and visible for many seconds after the cessation of the electric discharge.

These tubes (known from their constructor as "Geissler tubes"), it was to be remarked were not new inventions in themselves, but were here introduced as a novelty, because never before exhibited in any way in this city or its vicinity. The set of tubes employed were the property of Mr. Charles N. Bancker to whose kindness the Institute was indebted for the pleasure of inspecting them.

Under the head of *Chemistry* reference was made to the establishment by Meissner, of Ant-Ozone as an alotropic condition of Oxygen;—to the power which Ozone possesses of arresting vegetable growth and the relation between this growth and the putrefaction of animal matter;—to the delicate test for free Phosphorus furnished by its green coloration of the flame of hydrogen generated in its presence, especially when the temperature of the flame was reduced by the introduction of a solid good conductor, such as a mass of iron, lead or the like;—to the extraction of gold from poor ores by nascent Chlorine, &c.

Mr. Sellers then made some remarks upon the employment of the fluorescent properties of certain kinds of glass in the analysis of light, especially with reference to its photographic powers.

An outline was then given of the lecture on Light which it is proposed to deliver before the Institute at the Academy of Music, and on motion the meeting was adjourned.

HENRY MORTON, *Secretary.*

A Comparison of some of the Meteorological Phenomena of FEBRUARY, 1865, *with those of* FEBRUARY, 1864, *and of the same month for* FOURTEEN *years, at Phila., Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By J. A. KIRKPATRICK, A. M.

	February, 1865.	February, 1864.	February, for 14 years.
Thermometer—Highest—degree, .	56·00°	57·00°	70·00°
" date, .	26th.	28th.	23d, 1860.
Warmest day—Mean,	49·17	49·67	59·30
" " date,	26th.	28th.	25th, 1857.
Lowest—degree, .	3·50	4·00	—1·00
" date, . .	13th.	17th.	7th'55;8th'61
Coldest day—Mean,	12·00	7·00	4·50
" " date, .	13th.	17th.	6th, 1855.
Mean daily oscillation,	11·34	15·29	13·40
" " range, .	5·97	7·18	7·21
Means at 7 A. M., .	28·45	30·55	29·29
" 2 P. M., .	36·20	40·57	38·38
" 9 P. M., .	32·44	34·74	33·45
" for the month,	32·36	35·29	33·71
Barometer—Highest— Inches, .	30·322 in.	30·375 in.	30·671 in.
" date, . .	21st.	19th.	4th, 1863
Greatest mean daily press.	30·293	30·267	30·595
" date, . .	21st.	18th.	12th, 1857.
Lowest—Inches, . .	29·256	29·284	29·065
" date, . .	8th.	16th.	23d, 1853.
Least mean daily press.,	29·347	29·383	29·227
" date, . .	8th.	16th.	16th, 1856.
Mean daily range, .	0·205	0·182	0·220
Means at 7 A. M., .	29·872	29·792	29·916
" 2 P. M., .	29·849	29·742	29·869
" 9 P. M., .	29·875	29·796	29·902
" for the month,	29·865	29·777	29·896
Force of Vapor—Greatest—Inches,	0·326 in.	0·262 in.	0·549 in.
" date, .	26th.	1st.	16th, 1857.
Least—Inches, .	·044	·022	·013
" date, .	13th.	17th.	6th, 1855.
Means at 7 A. M.,	·124	·132	·138
" 2 P. M.,	·134	·124	·158
" 9 P. M.,	·137	·139	·156
" for the month,	·132	·132	·151
Relative Humidity—Greatest—per ct.,	100·0 per ct.	90·0 per ct.	100·0 per ct.
" date,	23d.	6th.	often.
Least—per ct.,	35·0	20·0	20·0
" date, .	24th.	22d.	22d, 1864.
Means at 7 A. M.,	75·1	70·1	78·4
" 2 P. M.,	61·4	46·6	63·1
" 9 P. M.,	71·7	63·8	75·1
for the month,	69·3	60·2	72·2
Clouds—Number of clear days,* .	8	10	8·1
" cloudy days, .	20	19	20·
Means of sky cov'd at 7 A. M.	58·2 per c.	50·7 per c.	61·4 per c.
" " " 2 P. M.	58·9	63·1	60·8
" " " 9 P. M.	61·8	47·6	48·2
" " for the month,	59·6	53·8	56·8
Rain and melted Snow—Amount,	5·491 in.	0·697 in.	2·927 in.
No. of days on which Rain or Snow fell,	10·	5·	10·
Prevailing Winds—Times in 1000,	N65°18′W·351	S80°41′W·379	N72°17′W·288

*Sky, one-third or less covered at the hours of observation.